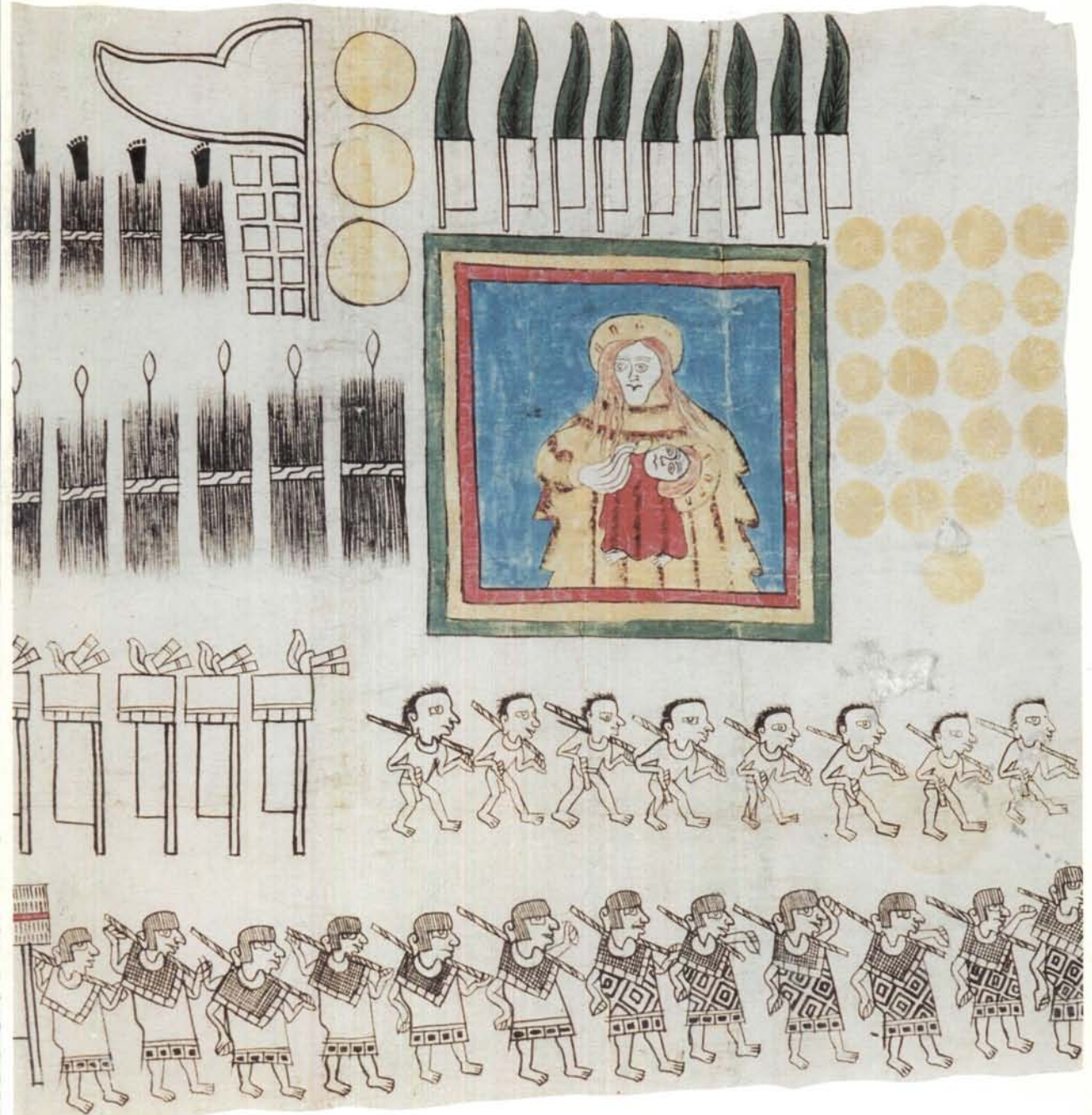
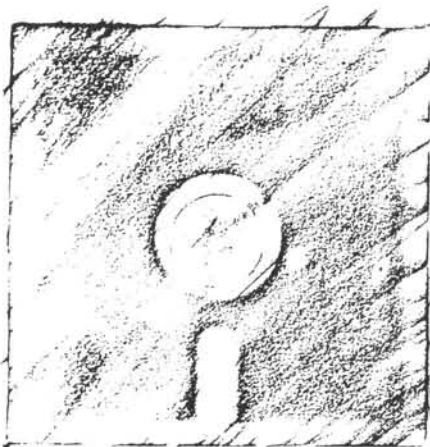


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The American Historical Review

AMERICAN HISTORICAL ASSOCIATION





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by Janice L. Reiff

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In This Issue

This issue features two *AHR Forums* on problems in American history, a recapitulation and defense of the work of historians who launched the new political history of the United States, and a summing up of some of the literature associated with the Quincentenary of Columbus's encounter with the New World.

Michael O'Malley leads off the first *AHR Forum* with an imaginative analysis of the language used in the nineteenth century to debate questions about the nature of race and questions about the nature of money and value. He finds many similarities. During Reconstruction, proponents of specie argued that gold contained an intrinsic value placed there by God, and the precious metal was therefore the only basis for a well-run economy. Many of the same people argued that African Americans lacked the intrinsic qualities needed for citizenship and that Congress could no more "legislate fitness into the negro" than it could legislate value into paper. Demands by black Americans for equality threatened assumptions about race and the economic system, and the response of many white leaders was to elaborate theories of racial and economic essentialism. O'Malley's point brings into question the idea that capitalism is a solvent that reduces everything to price. Instead, he sees the idea of specie being deployed as a means of stabilizing and defining difference.

Nell Irvin Painter applauds O'Malley's effort to enrich historical analysis with attention to language, but she expresses concern about his limited view of relations between race and money. She offers four examples: first, a failure to see the literal equation of blacks and property before the Civil War; second, a neglect of the gendered nature of the language; third, the problem of other discourses about black manhood that were in conflict with those O'Malley cites; and, fourth, a failure to note that the realm of money furnishes a pervasive source of metaphor in Western societies. Painter also rejects the notion that the freer a market society becomes the more it requires new types of constraints, much less a new type of fixity based in racial difference. Painter contends that O'Malley's reliance on European poststructuralist analysis may have narrowed his vision, for the poststructuralists neglected to theorize race or, curiously, even the colonialism that was central to the times in which they were writing.

Michael O'Malley offers a brief rebuttal.

American conservatism is the subject of the second *AHR Forum*. The lead author, **Alan Brinkley** points out that historians have been slow to recognize the important place of the Right in modern America. He seeks to explain this omission by examining the analytical schemes most used by scholars in interpreting recent U.S. history. The majority of historians in this field are liberal or leftist and tend to use analyses premised on progressive assump-

tions, analyses that accordingly pose difficulties to explaining the survival of substantial numbers of Americans who reject progressive ideas and norms. Brinkley also argues that the great diversity of views within the Right (from libertarian to rigidly normative) has complicated the task of explaining the rise of conservative political power.

Susan M. Yohn sees problems in Brinkley's use of fixed categories of liberal and conservative in assessing the ideological sympathies of individuals and groups. Not only are the views of many people not easily captured by such labels but people and organizations also shift their sympathies and political stances over time, as was the case with the women's home mission in the Southwest that Yohn has studied. Yohn is also critical of the notion that social historians have neglected the Right. Though focusing primarily on the Left, these historians necessarily include the Right in their stories as a crucial element in the shaping of leftist politics.

Leo P. Ribuffo, too, questions Brinkley's premise that the Right has been neglected. The literature on conservatism, Ribuffo contends, is more extensive and better than Brinkley allows. The problem is that it has not been integrated into the larger story, nor has its complexity and its negotiation with other political traditions been sufficiently appreciated. Ribuffo concludes his comment with a survey of twentieth-century American politics in which he sketches a model of how conservatism might be integrated into a master narrative.

Alan Brinkley responds to his critics.

Ronald P. Formisano argues that the "ethnocultural interpretation" of political parties and voting in nineteenth-century America is largely the invention of the critics of the new political history. The two historians most often associated with the origin of the new political history in this country, Samuel P. Hays and Lee Benson, did not create or advocate an ethnocultural interpretation and had this label attached to them by others who misunderstood their analyses (and those of their followers) and vulgarized them into a reductionist notion that politics were determined solely by voters' ethnic and religious affiliations. In support of his argument, Formisano compares the writings of the new political historians with the construction given them by their critics—and in the process surveys much of the recent literature on U.S. political history.

The *AHR* did not do a special issue on the Columbus Quincentenary, in view of its thorough coverage by other publications. We wanted nevertheless to follow the celebration with an assessment of the work that emerged from the anniversary. We asked prize-winning book author **Ida Altman** and her collaborator **Reginald D. Butler** to evaluate the new literature on early transatlantic contacts. Altman and Butler chose to examine the literature

about two sets of relationships: contacts between Europeans and indigenous Americans and between Europeans and Africans. Despite their interdependence, these two stories are usually studied separately. In bringing together consideration of forms and consequences of contacts of representatives of all major groups, Altman and Butler look at the dynamics at work in the entire Atlantic basin rather than focusing on one region or on the colonizing efforts of a single nation. They try to find in the diverse works under review some general explanation for the success of early modern Europeans in imposing their political and social institutions on indigenous Americans and Africans in the Americas.

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Specie and Species: Race and the Money Question in
Nineteenth-Century America

MICHAEL O'MALLEY

IN *New York by Gas-light*, AN URBAN EXPOSÉ OF 1850, George Foster describes two “magnificently attired” prostitutes beckoning to a country rube. “But for their large feet and vulgar hands,” he tells us, “they would be taken for queens or princesses.” Close scrutiny reveals the essential difference that market goods obscure.¹ Writing at the same time, Edgar Allan Poe offered another version of the urban exposé in his short story “The Man of the Crowd.” Poe’s narrator sits in a café watching “the tumultuous sea of human heads” roll by. At first, his observations take “an abstract and generalizing turn.” Soon, however, “I descended to details, and regarded with minute interest the innumerable varieties of figure, dress, air, gait, visage, and expression of countenance.” He differentiates between men of various ranks by their habits of dress and styles of walking, finally lighting, like Foster, on physical peculiarities. For example, he terms gamblers “easily recognizable” by “a certain sodden swarthy complexion, a filmy dimness of eye, and pallor and compression of lip”; he also detects “a more than ordinary extension of the thumb in a direction at right angles to the fingers.” Like Foster, Poe’s narrator believes he has discerned an essential physical difference between categories of people in public.²

Essentialism—the search for fundamental, intrinsic, “essential” categories of being or laws of nature—characterized much of nineteenth-century public discourse. Samuel Morton, for example, helped establish the “American School” of anthropology by patiently sifting birdseed or shot into human skulls. The results, by giving a measure of cranial capacity, allowed him to rank different races in terms of intelligence and to offer scientific proof of the relationship between physical appearance and essential character and ability.³ Following Morton and Louis Aggasiz, American anthropologists argued for polygeny, or a

I would like to thank Thomas Bender, Ann Fabian, Reid Mitchell, Michael Rogin, and the anonymous readers for the *AHR*, as well as Peter Antelyes, Miriam Cohen, Clyde Griffen, James Merrell, and Donald Olsen.

¹ George G. Foster, *New York by Gas-light and Other Urban Sketches* (1850; rpt. edn., Berkeley, Calif., 1990), 70.

² Edgar Allan Poe, “The Man of the Crowd,” in Thomas Ollive Mabbott, ed., *Collected Works of Edgar Allan Poe* (Cambridge, Mass., 1978), vol. 2: 507–09.

³ Samuel George Morton, *Crania Americana* (Philadelphia, 1839).

separate line of evolutionary descent for each race.⁴ Pioneering work in medicine and psychology also assumed an essential difference between men and women, in which women's physical difference translated into differences of temperament, ability, and intellect.⁵ Essentialist theories of racial or sexual difference pervaded American society at all levels. Ironically, in a free society whose formal political rhetoric insisted that the prospect of self-making shone equally before all, nature had apparently marked some people with an essential, irreducible, non-negotiable, and intrinsic difference.

In both Europe and America, nineteenth-century science rested on the proposition that all behavior, the world itself, was governed by natural laws.⁶ If this included behavior caused by intrinsic racial or genetic character—genius and idiocy, criminality and altruism—it also explained economic incompetence as well as business genius. At its worst, the insistence on natural law bolstered the soothing fatuities of Social Darwinism and essentialist justifications for economic and legal inequality.

Nineteenth-century economics shared this obsession with fundamental and essential laws, especially in the popular discussions of the nature and value of money that filled newspapers, magazines, textbooks, and the *Congressional Record* throughout the century. If physical signs offered one marker of essential identity, possession of money and the virtues it implied offered another. "It is no derogation to the Boston aristocracy," claimed a guidebook to Boston society in 1846, "to say that it rests upon money. Money is something substantial. Everybody knows this and feels it. Birth is a mere idea which grows every day more and more intangible."⁷ But what if the money itself had no fixed character? What if it represented no more than social convenience, or optimism, or paper promises? The answer was the gold (and silver) fetish, the nineteenth-century political economy's insistence on a specie economy and the intrinsic value of precious metal. The essential value of specie, like the essential character of certain races or occupations, helped resolve the ambiguity of identity in public by a resort to "natural facts."

This article compares the language of race to the language of money. It pays special attention to Reconstruction, when heated debates over citizenship for African Americans coincided with equally hot debates over the nature of money. It argues in part that discussions of money used the same terms, the same analogies and assumptions, as discussions of race. This discursive similarity suggests a new way to understand the political economy of "essential" identity.

There is, of course, much more to nineteenth-century philosophy than essentialism. If the dominant branch of nineteenth-century thought was built on essentialism, fixed character, and natural law, other philosophies insisted on self-making, the social creation of value and identity, and the rule of human law

⁴ Stephen Jay Gould, *The Mismeasure of Man* (New York, 1981), 30–72.

⁵ For a summary of the Anglo-American literature on women in the nineteenth century, see Cynthia Eagle Russett, *Sexual Science: The Victorian Construction of Womanhood* (Cambridge, Mass., 1989).

⁶ Russett, *Sexual Science*, 5–7.

⁷ "Our First Men: A Calendar of Wealth, Fashion and Gentility" (Boston, 1846), quoted in Simon Schama, *Dead Certainties (Unwarranted Speculations)* (New York, 1991), 86.

over laws of nature. These more “relativistic” or social theories of value and identity—of which the greenback–Radical Republican synthesis during Reconstruction constitutes a major example—gained the upper hand in the early twentieth century, although they remain contested today just as they were 150 years ago.⁸ This article also explores the tension between the ideal of freedom in self-making and the comfort of fixed identity as it appeared in debates about money, value, citizenship, and race between 1830 and 1900, concluding that the resort to racial and economic essentialism that characterized American political economy before 1900 was rooted in the logic of the free market itself.⁹

The larger critique of free market liberalism that this analysis implies will only be suggested here. The focus of a book-length study in progress, it stems in part from recent “postmodern” work in the history of the human sciences, especially the provocative insights of Michel Foucault, who first posited a similarity between political economy and biological taxonomy in *The Order of Things*.¹⁰ Much critical theory of the past twenty years, whether or not it suffers the label “postmodern,” treats individualism and race as socially constructed notions rather than essential facts of being. It pays special attention to the ways societies interpret the natural world and the relationship between themselves and “natural facts.”¹¹ In an essay

⁸ Carl N. Degler, *In Search of Human Nature: The Decline and Revival of Darwinism in American Social Thought* (New York, 1991), 59–127; Richard Hofstadter, *Social Darwinism in American Thought* (Boston, 1955), 201–04; Thomas L. Haskell, *The Emergence of Professional Social Science* (Urbana, Ill., 1977); and Haskell, “Reply to Davis and Ashworth,” in Thomas Bender, ed., *The Antislavery Debate: Capitalism and Abolitionism as a Problem in Historical Interpretation* (Berkeley, Calif., 1992), 257–58.

⁹ The debate over capitalism, antislavery, and “the humanitarian sensibility” published in the *AHR* in October 1987 examined the relations between capitalism, class interest, and abolition. For all their rigor and breadth, none of the essays in the *Review* or the published volume that extended the debate paid any significant attention to the fact that slavery was primarily *racial*, that its assumed virtues or horrors all revolved around the problem of racial difference. Did black people possess the same essential individual traits, the same capacity for self-interest and self-ownership that whites did? If training might inculcate these virtues, then did the virtues themselves really represent the sort of “natural facts” they appeared to? The essays overlook the problem, common to both political economy and political philosophy, of difference—the need to establish formal laws explaining the meaning of difference in the market and the need to puzzle out stable meanings for human diversity. I refer here to the exchanges between Thomas Haskell, David Brion Davis, and John Ashworth as reprinted in Bender, *Antislavery Debate*.

¹⁰ Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (New York, 1973), 175, describes the relation between political economy, taxonomy, and formal systems of grammar: “All wealth is *coinable*; and it is by this means that it enters into *circulation*—in the same way that any natural being was *characterizable* and could thereby find its place in a *taxonomy*; that any individual was *nameable* and would find its place in an *articulated language*; that any representation was *signifiable* and would find its place, in order to be *known*, in a *system of identities and differences*.” See also Foucault, *Discipline and Punish: The Birth of the Prison*, Alan Sheridan, trans. (New York, 1977); and *The History of Sexuality*, Vol. 1, Robert Hurley, trans. (New York, 1978).

¹¹ Anthropological works of special relevance to cultural history include Mary Douglas, *Natural Symbols: Explorations in Cosmology* (London, 1970); Clifford Geertz, *The Interpretation of Cultures* (New York, 1973); and Geertz, *Local Knowledge* (New York, 1983); see also the essays collected in Henry Louis Gates, Jr., ed., “Race,” *Writing and Difference* (Chicago, 1986); and Gates and K. Anthony Appiah, guest editors, “Multiplying Identities,” *Critical Inquiry*, 18 (Summer 1992): 625–29. On money in the late nineteenth century, see Walter Benn Michaels, *The Gold Standard and the Logic of Naturalism: American Literature at the Turn of the Century* (Berkeley, Calif., 1987). Historians drawing on this work include Carroll Smith-Rosenberg, *Disorderly Conduct: Visions of Gender in Victorian America* (New York, 1986); Mary P. Ryan, *Women in Public: Between Banners and Ballots, 1825–1880* (Baltimore, Md., 1990); Michael Paul Rogin, *Fathers and Children: Andrew Jackson and the Subjugation of the American Indian* (New York, 1975); Richard Slotkin, *The Fatal Environment: The Myth of the Frontier in the Age of Industrialization, 1800–1890* (New York, 1985); G. J. Barker-Benfield, *Horrors of the Half-Known Life:*

of particular relevance to this study, for example, Joyce Appleby has connected Locke's focus on intrinsic individual rights to his defense of the intrinsic value of specie.¹²

Consider, in this light, the common root of the word "specie," meaning coin or precious metal, and the word "species," meaning "of a kind." Both originated in attempts to create stable classificatory systems, to find scientific and objective definitions of the meaning of difference. Specie, like species, signified a belief in irreducible difference and final identity, or at least the dream of finding it. Think also of the extremely common metaphors comparing money, circulating through the nation, to blood circulating through the body. Thomas Hobbes understood money as the blood in Leviathan's body, and the metaphor served pro-slavery Southerner John C. Calhoun equally well nearly two hundred years later. "The currency of a country is to the community," Calhoun wrote in 1837, "what the blood is to the body . . . indispensable to all the functions of life."¹³ What is race but a theory of purity in blood?

In this essay, I will not attempt to map the relation between individual ideology and material interest in the era of Reconstruction, work that has been convincingly done already.¹⁴ Rather, I wish to compare two different political debates in order to reveal their shared assumptions—a common resort to nature of which contemporaries may not have been explicitly aware but which their language reveals. I do not therefore seek to explain why specific individuals during Reconstruction voted a certain way with or against their interests but, by examining the paradoxes inherent in free market liberalism, help explain why Reconstruction failed and why essentialism proved so seductive.

A dizzying market freedom demanded a corresponding area of certainty, especially when African Americans began taking that market freedom seriously. Historians readily acknowledge capitalism's corrosive effect on traditions and certainties that once seemed as fixed as granite. Inherited positions, the customs of generations, the framework of beliefs Europeans had clung to for long ages, all collapsed before the universal power of money. But if many seemingly solid

Male Attitudes toward Women and Sexuality in Nineteenth-Century America (New York, 1976); Thomas Laqueur, *Making Sex: Body and Gender from the Greeks to Freud* (Cambridge, Mass., 1990); and Laqueur and Catherine Gallagher, eds., *The Making of the Modern Body: Sexuality and Society in the Nineteenth Century* (Berkeley, Calif., 1987); Sander L. Gilman, *Difference and Pathology: Stereotypes of Sexuality, Race, and Madness* (Ithaca, N.Y., 1985).

¹² Joyce Oldham Appleby, "Locke, Liberalism, and the Natural Law of Money," *Past and Present*, 71 (May 1976): 68. I am greatly indebted to Professor Appleby's analysis in this article.

¹³ Thomas Hobbes, *Leviathan*, Michael Oakeshott, ed. (Oxford, 1955), 164–65; Calhoun quoted in Lacy K. Ford, *Origins of Southern Radicalism: The South Carolina Upcountry, 1800–1860* (New York, 1988), 93.

¹⁴ In addition to the two standard works on the subject, Irwin Unger, *The Greenback Era: A Social and Political History of American Finance, 1865–1879* (Princeton, N.J., 1964); and Robert Sharkey, *Money, Class, and Party: An Economic Study of Civil War and Reconstruction* (Baltimore, Md., 1959), see also Terry L. Seip, *The South Returns to Congress* (Baton Rouge, La., 1983), 171–218; Michael Les Benedict, *A Compromise of Principle: Congressional Republicans and Reconstruction, 1863–1869* (New York, 1974), 48–56; Richard Franklin Bensel, *Yankee Leviathan: The Origins of Central State Authority in America, 1859–1877* (Cambridge, 1990), 303–65; William Gillette, *Retreat from Reconstruction, 1869–1879* (Baton Rouge, 1979), 186–210. Bensel's recent work ties the establishment of the gold standard and the collapse of racial equality to the emergence of central state authority and the need for stability. My analysis here confirms his conclusions through different means.

things melted into air, other more nebulous ideas grew more certain as capitalism advanced. The belief in an essential difference between white and black is one of those ideas, and in this sense free market liberalism seems self-defeating or self-subverting. If it preached freedom in self-making, it also gave racial difference a fixed and non-negotiable meaning. The shared language of race and money suggests that the freer market society became, the farther its promises extended, the more it demanded racial categories that resisted exchange or renegotiation. It helps explain why the resurgent racism of the 1890s was accompanied by a political obsession with gold and silver and the "intrinsic value" of specie. It repeats the question Edmund S. Morgan raised with such force in *American Slavery, American Freedom*: can American liberalism ever rid itself of the racial bogey it helped to create?¹⁵

ADDRESSING THIS QUESTION requires exploring the ambiguity of value and identity in antebellum political economy. Before the Civil War, few laws restrained American banking, and no uniform national currency circulated. Although the colonies had experimented lavishly with paper money, orthodox opinion held that the "intrinsic value" of gold made it the "natural" money of the new nation.¹⁶ Only gold and silver served as legal tender, so, theoretically, the amount of money in the United States could not exceed the amount of precious metal—specie—the country possessed. But antebellum enterprisers chafed at this restriction.¹⁷ Too little money kept interest rates high, and the limited supply of gold stayed mostly in the Northeast, retarding commerce elsewhere. Paper money, issued by a local bank in excess of actual specie deposits, could serve perfectly well if everyone believed in future prosperity: eventually, the local economy would catch up with the dreams of its boosters, and the value of paper money would stabilize as gold filled the bank's vaults.

Local banks, especially but not exclusively in the West, began issuing paper notes based far more in speculative optimism than in actual reserves of "real money." Americans doing business during this period used thousands of different bank notes as money, some born of august and marble-pillared institutions, some of institutions of a more doubtful nature, the doubt usually increasing as the notes traveled farther from home. Deposit \$100 in gold at a bank in Georgia, taking notes from the Georgia bank in return, and begin traveling north, and you might

¹⁵ Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975).

¹⁶ See, for example, Alexander Hamilton, "Treasury Report on the Establishment of a Mint, January 28, 1791," reprinted in Herman E. Krooss, ed., *Documentary History of Banking and Currency in the United States*, 4 vols. (New York, 1983), esp. 100, 108.

¹⁷ Especially after Jackson's defeat of the Second U.S. Bank. Although the collapse of the bank is often described as setting off a banking and currency boom, the lavish use of paper money predated the Bank War and in fact helped inspire it. See Bray Hammond, *Banks and Politics in America, from the Revolution to the Civil War* (1957; rpt. edn., Princeton, N.J., 1991); and Alonzo Barton Hepburn, *History of Coinage and Currency in the United States and the Perennial Contest for Sound Money* (1903; rpt. edn., New York, 1968), 89–90, speaking of the era from 1812 to 1817: "corporations and tradesmen issued currency . . . Even barbers and bartenders competed with banks in this respect . . . [N]early every citizen regarded it as his constitutional right to issue currency." An exaggeration, Hepburn's statement does convey the perceived loose nature of the money system.

reach Boston with a wallet full of worthless notes.¹⁸ In every city or town, brokerage houses offered to trade notes from a distant bank, at a discount, for notes of a local sanctum of fiscal probity. The system resembled modern foreign currency exchanges, except it was vastly more complicated and more prone to ambiguities about the nature of value.¹⁹

Besides a profusion of notes, all circulating at different discounts, merchants also experienced rampant counterfeiting. The diversity of the circulating medium, coupled to relatively crude printing technology, made this a "golden age" of bogus bills. Some writers estimated that at times roughly 40 percent of the money in circulation was counterfeit.²⁰ Any criticism of this figure should consider the fuzzy line between counterfeit and genuine notes. Notes that a bank could not, in its most fevered dreams of prosperity, ever hope to redeem in specie bore considerable resemblance to their illegitimate cousins—"the handsome counterfeits of the currency put forth by the old time 'Coney men' were not only equal in artistic appearance," wrote detective Allan Pinkerton, but "based upon an almost equivalent in value. It was a popular remark among men of business that they preferred a good counterfeit on a solid bank to any genuine bill upon the shyster institution."²¹ Counterfeiting called attention to the dubiousness of much American business and to the slippery, shifting, mutable quality of value and identity in a free market society.

A unique kind of periodical flourished in this chaos: the monthly or weekly "counterfeit detector." These journals, dozens of which served every major city, gave the discount rate prevailing on different bank notes and listed known counterfeits. Merchants receiving a bank note in trade consulted the "counterfeit detector" to check its authenticity, much as, before on-line credit card scanners, clerks checked a thick paperbound list of stolen credit cards. But enterprising bankers quickly realized the advantages of having their notes listed as "safe" and secure. In exchange for a "consideration," the editor of a counterfeit detector might overlook some difficulty with a given bank's notes, listing it as stable when in fact the issuing bank had "busted." Or the same editor might name a competing bank's notes suspect. "Counterfeit counterfeit detectors" soon appeared. We discover our competitors, one began, "tampering with the engravers—puffing their swindles, in advertisements—occasionally buying up an editor . . . and often fastening on the green publishers of Bank Note Lists. We have felt the slimy skin

¹⁸ William H. Dillistin, *Bank Note Reporters and Counterfeit Detectors, 1826–1866* (New York, 1949), 3–4. This remarkable monograph gives a detailed picture of the day-to-day mechanics of American business in the period.

¹⁹ John Kenneth Galbraith, *Money: Whence It Came, Where It Went* (Boston, 1975), 88–89: "By the time of the Civil War, the American monetary system was, without rival, the most confusing in the long history of commerce and associated cupidity."

²⁰ This figure appears in writings on counterfeiting by members of the American Numismatic Society but without primary source attribution. See Lynn Glaser, *Counterfeiting in America: The History of an American Way to Wealth* (New York, 1968), 104–05; Murray T. Bloom, *Money of Their Own* (1957; rpt. edn., Port Clinton, Ohio, 1982), 101–03. When judging its accuracy, one should keep in mind the dubious nature of much of the paper money in circulation. Business journals such as *Niles Weekly Register* or *Banker's Magazine* make counterfeiting seem ubiquitous, as does the extraordinary number of "counterfeit detectors" described in Dillistin, above, n. 18. I believe that the figure is more or less accurate, again depending on how speculative paper issues by local banks are regarded. See the table of banks and counterfeits given in Hepburn, *History of Coinage and Currency*, 160.

²¹ Allan Pinkerton, *Thirty Years a Detective* (New York, 1884), 518.

of a Shinplaster Banker grasping us, but we much prefer a libel suit to any such contact."²²

In their inevitable failure to prevent the thing they warned against, counterfeit detectors paralleled the Jacksonian-era etiquette manuals and guides to sincere behavior described in Karen Halttunen's *Confidence Men and Painted Women*. Facing a supposedly open society where anyone could become anything, middle-class Americans published guides to manners that promised to elaborate the difference between the genuine and the false, the sincere and the corrupt. By giving formal criteria for identifying the genuine, counterfeit detectors only made the counterfeiter's job easier, just as etiquette manuals that elaborated the marks of sincerity only made sincerity easier to fake.²³ Flourishing at the same time, these two kinds of publication highlighted a persistent anxiety about the instability of value and identity in American society.²⁴

"Wildcat banks" live in legend as the worst offenders against "genuine" value. The term's origins are unclear, but we can guess: like wildcats, they appeared and vanished quickly, they resided in remote locales, they turned and bit when cornered.²⁵ Wildcat banks removed themselves as far as possible from ordinary foot traffic, hoping that, once their notes circulated, no one wishing to redeem them for legal money—gold and silver—would ever find the bank. Critics complained that the meager specie reserves of one bank often rushed, one step ahead of the overworked banking inspector, to the next branch of the same institution. "Gold and silver flew about the country with the celerity of magic," complained the Michigan banking commissioner in 1839, "its sound was heard in the depths of the forest, yet, like the wind, one knew not whence it came or whither it was going."²⁶

These complaints came from fiscal conservatives, from established creditors who disliked payment in paper money and eastern bankers who resented losing control of the money supply. In a different light, "wildcat banks" embodied American enterprise and optimism, allowing striving empire builders to escape

²² Thompson's *Bank Note and Commercial Reporter*, August 15, 1853, quoted in Dillistin, *Bank Note Reporters*, 87. See also Arthur A. Smith, "Bank Note Detecting in the Era of State Banks," *Mississippi Valley Historical Review*, 29 (December 1942): 371–86.

²³ Karen Halttunen, *Confidence Men and Painted Women: A Study of Middle-Class Culture in America, 1830–1870* (New Haven, Conn., 1982). See also John F. Kasson, *Rudeness and Civility: Manners in Nineteenth-Century Urban America* (New York, 1990).

²⁴ Nineteenth-century Americans expressed a consistent ambivalence about imitation, a fondness for fakery and reproduction mixed with a hunger for the real thing. Victorian Americans elaborated forms of imitation but also scorned fraud and hypocrisy. A growing historical literature connects this concern to the explosion of mass-produced consumer goods while also arguing that the theoretical dream of a classless society, limitless in its potential for self-making and upward mobility, produced a corresponding anxiety about the stability of character and place. See Ann Vincent Fabian, *Card Sharps, Dream Books, and Bucket Shops: Gambling in Nineteenth-Century America* (Ithaca, N.Y., 1990); Neil Harris, *Humbug: The Art of P. T. Barnum* (Boston, 1973); Miles Orvell, *The Real Thing: Imitation and Authenticity in American Culture, 1880–1940* (Chapel Hill, N.C., 1989); and Philip Fisher, "Democratic Social Space: Whitman, Melville and the Promise of American Transparency," in Fisher, ed., *The New American Studies: Essays from "Representations"* (Berkeley, Calif., 1991).

²⁵ According to some accounts, the notes of Michigan banks, which were especially shaky, carried an imprint of a wildcat, and the bad reputation of this currency led to the common term for bad paper. See Hepburn, *History of Coinage and Currency*, 138. See also Hugh Rockoff, *The Free Banking Era: A Re-examination* (New York, 1975), 5.

²⁶ Hammond, *Banks and Politics*, 601.

the tight monetary grasp of eastern elites. We might roughly compare wildcat bank notes to the "junk bond" issues of the 1980s, which, from one perspective, allowed marginal enterprisers to raise money for expansion or, from another, allowed unscrupulous speculators to grossly inflate the value of doubtful properties. Like critics of junk bonds, enemies of wildcat banks willingly overlooked the speculative uncertainties at the heart of their own respectability and took refuge in notions of genuine or "intrinsic" value—the kind of value supposedly located in specie, or oil paintings, wood paneling, and old family gentility.²⁷ If a frontier bank lasted long enough to see its region settled and prosperous, then its vaults might fill with specie and it too could denounce newcomers.²⁸

As an insult, the term "carpetbagger" first appeared in connection with these "wildcat" banks. Today, the word describes Northerners who came south after the Civil War to "fatten on our misfortune." Manipulating the "ignorant" Negro vote with valueless greenback dollars, "carpetbag governments" supposedly fostered unprecedented corruption, drove the former southern states into debt, and then fled. Carpetbaggers misunderstood the Negro, claimed those who overturned "carpetbag rule," and by artificially maintaining the freedmen in power they bankrupted the South and degraded the whites. The "redemption" of the South by white men restored the natural order of things. Although recent scholarship revises this portrait substantially, scholars have paid no attention to the origins of the term "carpetbagger" itself, usually simply referring to the famous valise, made of carpeting material, that supposedly carried all the Northerner's worldly goods.²⁹ But the earlier financial context of "carpetbagger" has special relevance for my analysis of the politics of Reconstruction.

In the context of wildcat banking, the term described someone who returned a bank's notes to their birthplace, demanding their equivalent in specie. "No piece of information could be more annoying to the antebellum banker," remembered a North Carolina bank examiner, "than the news that a stranger was in town with a suspicious-looking carpet-bag in his hand; for it generally contained a bag full of notes of his bank for redemption." If he could not "redeem" the notes, his bank

²⁷ A good early example might be Senator Thomas Hart Benton, who, having prospered, like Jackson himself, in the speculative western economy, later became a tireless exponent of the intrinsic value of gold and a specie basis for money. See Rogin, *Fathers and Children*, chaps. 3, 9. I am greatly indebted to Professor Rogin's interpretation of Jacksonian political economy. The theme of false value on the Wall Street of the 1980s is nearly ubiquitous in the vast number of "exposés" that accompanied the widely reported reappraisal of greed in the early 1990s. See Oliver Stone's film *Wall Street* (1987) or Michael Lewis's best seller *Liar's Poker: Rising through the Wreckage on Wall Street* (New York, 1989).

²⁸ Rockoff, *Free Banking Era*, 66–68. Rockoff concludes that traditional accounts have much exaggerated both the extent of "wildcat banking" and its evil effects, while Susan Previant Lee and Peter Passell, *A New Economic View of American History* (New York, 1979), 108–29, dismiss accounts of wildcat banking as the products of a now outmoded "Soundness School." They claim that use of paper bills actually declined during the period in question. But whether or not "wildcat" banking actually did create the chaos that was claimed for it is largely irrelevant here. The widespread anxiety over unstable value, and the attraction of stories of "wildcat banks" to both contemporaries and later historians, indicates a deeper uneasiness about value and identity at the heart of liberal capitalism.

²⁹ Richard Nelson Current, *Those Terrible Carpetbaggers* (New York, 1988); William C. Harris, "Creed of the Carpetbaggers," in *Journal of Southern History*, 40 (May 1974): 199–224; Lawrence N. Powell, "The Politics of Livelihood: Carpetbaggers in the Deep South," in J. Morgan Kousser and James M. McPherson, eds., *Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward* (New York, 1982), 315–48; and the account in Eric Foner, *Reconstruction, 1863–1877* (New York, 1988).

went “bust.” But the word also described someone who, with a carpetbag stuffed full of dubious “wildcat” notes, went forth to “puff” them into circulation. Congressman William D. Kelley recalled that “nearly every specie-basis bank had its carpet-bagger—a fellow it sent out with notes by the carpet-bag full into some distant State to get them into circulation there.” In this sense, the carpetbagger served as the *agent* of the doubtful bank, rather than its nemesis.³⁰

This etymology gave “carpetbagger” two meanings, both connected to inflated value. On the one hand, the word represented inflated value called to account; on the other, it described inflated value actively stalking the local economy in respectable guise. The word’s reappearance during Reconstruction, as an insult hurled at Northerners who used greenback dollars to help form biracial governments, connected the enterprise of African-American equality to fraudulent or counterfeit bills. It connected civil rights to speculative endeavors of dubious character, and it connects the racial history of Reconstruction to the other great issue of the time, money.³¹

The word “intrinsic” links the two issues. If money’s value was intrinsic, governed like the value of gold by laws of nature rather than society, then so, too, individuals had an intrinsic, inalienable character, governed by natural law. But if money and value came from culture, from civil authority, then civil authority could create value and identity at will. By extension, did law and culture confer citizenship, or was it a natural right, a function of simply “being”? Although these questions had long haunted American political economy, they gained a renewed urgency during Reconstruction, when the meaning of racial difference, its value, was renegotiated. Former slaves, supposedly consigned by nature to the bottom rail, began acting as free agents in the marketplace. When ex-slaves threatened to erase the meaning of the difference between black and white by prospering politically and financially in a greenback economy, Gilded Age Americans responded with “natural law” arguments about “intrinsic” value that described both the nature of money and the nature of racial difference. As African Americans began pressing their claims for the right to “self-making,” theories of racial and sexual difference—of intrinsic value—grew more attractive and more persuasive. By the end of the period, the greenback position, the ideal of socially,

³⁰ John Jay Knox, *A History of Banking in the United States* (New York, 1903), 747; Dillistin, *Bank Note Reporters*, 63–65. Kelley, a “greenbacker” who regarded specie-basis money as unnecessary, claimed that the carpetbagger would travel to another state, buy goods with notes from his bank, then, in exchange for a “consideration,” carry back a carpetbag full of notes from a local bank, to “puff” them into circulation in his home state. This reciprocal relation suggests how the word could have both meanings: a more hostile carpetbagger could carry a valise filled half with his notes and half with notes from the place he was traveling to. He could then get his notes into circulation while presenting the other bank’s notes for “redemption” and carry the “redeeming” gold from the competing bank back to build his own bank’s reserves—enterprise indeed! Given enough banks, this relationship of musical chairs could go on indefinitely, each bank hoping to avoid being caught short when the tinkling music of circulation stopped at the demand for redemption. See also *Congressional Record*, 43d Congress, 1st session (April 8, 1874): 2931.

³¹ On the etymology of “carpetbagger,” see also Albion Tourgé, *A Fool’s Errand* (1879; rpt. edn., New York, 1966), 176–80. Tourgé also describes antebellum “carpetbaggers” as agents of doubtful banks, purveyors of “wildcat money,” and claims the word was first applied to Union Army soldiers by an editor who was himself a carpetbagger—perhaps an expression of the North’s ambivalence regarding equality.

democratically created value, had become literally unthinkable for the majority of American voters, as unthinkable as the ideal of racial equality.

THE UNION ISSUED \$450 million in "legal tender" greenbacks during the Civil War. Backed by nothing more than federal authority, the notes had no intrinsic value. Under the National Banking Act of 1863, the Union also taxed state bank notes out of existence, creating a uniform currency for the first time. Treasury Secretary Salmon P. Chase called this innovation "of perhaps equal value with the abolition of slavery, produced also by the war." A uniform currency circulated from Maine to Oregon, for the benefit of a now uniformly free labor pool.³² Practically from the date of issue, the greenbacks began depreciating relative to gold. But the North enjoyed substantial prosperity during the war, due partly to the loosening of credit that resulted from an increase in the money supply. After the war, the greenbacks loomed as a potentially explosive issue, here simplified. Retire the greenbacks, that is, "contract" the money supply, and interest rates would rise while prices fell. Keep the green bills circulating, or even increase their circulation, and prices would soar while interest rates fell. Debtors gained from the greenbacks; creditors suffered. Those with money to lend usually preferred gold, which they identified with natural law.³³

"Gold and silver," insisted Treasury Secretary Hugh McCulloch in 1865, "are the only true measure of value." This truthfulness came from God and nature: "I myself have no more doubt that these metals were prepared by the Almighty for this purpose, than I have that iron and coal were prepared for the purposes for which they are being used."³⁴ Only nature, not government or society, could create value and identity. According to economist Amasa Walker, "the true standard of value exists in nature . . . governments have rightfully nothing to do with it."³⁵ Theorists of intrinsic value cited nature's law, unaltered by legal enactment. Deploring greenbacks, Henry Adams called "the law of legal tender . . . an attempt by artificial legislation to make something true which was false."³⁶

Just as no legal enactment could create value, no government could affect the Negro's nature. "No legislation of Congress can elevate or improve the physical, moral or intellectual condition of the negro," maintained Senator George Vickers of Maryland in 1869. "We cannot legislate into them any fitness or qualifications which they do not now possess."³⁷ As McCulloch imagined natural mineral hierarchies, racists imagined natural social hierarchies, claiming "it is by the

³² Chase quoted in Lois Craig, *The Federal Presence: Architecture, Politics and Symbols in United States Government Building* (Cambridge, Mass., 1978), 173.

³³ This is, of course, a gross simplification of the historical issues. For a more complicated picture, see Unger, *Greenback Era*; and Sharkey, *Money, Class, and Party*; as well as Bensel, *Yankee Leviathan*, chaps. 4 and 5. For a view of the effects on labor, see Foner, *Reconstruction*, 478–79, 518; and David Montgomery, *Beyond Equality: Labor and Radical Republicans, 1862–1872* (New York, 1967).

³⁴ Hugh McCulloch, *Men and Measures of Half a Century* (New York, 1888), 201.

³⁵ Amasa Walker, "Governmental Interference with Standards of Value," *Bankers Magazine* (April 1867): 725.

³⁶ Henry Adams and Francis A. Walker, "The Legal Tender Act," *North American Review*, 110 (April 1870): 321.

³⁷ *Congressional Globe*, 40th Congress, 3d session (February 5, 1869): 911.

standards of nature that we are to determine whether the radical task of negro equality is practicable."³⁸ McCulloch himself wrote, "it is very evident to me that at the time of their enfranchisement they were not, are not yet, and probably never will be, qualified to properly control the government."³⁹ The white race rules "by virtue of its intrinsic strength," insisted James Pike, while Negroes rule South Carolina "by means of an alien and borrowed authority . . . it is not the rule of intrinsic strength; it is the compulsive power of Federal authority in Washington." Under this rule, political identity and economic value collapsed into each other, until "the vote of any Negro in the State is worth as much as a South Carolina bond." Pike equated the ex-slave, inflated to citizenship, to inflated bonds.⁴⁰

Against "bullionist" arguments about the intrinsic value of precious metal, some Americans advanced the "producerist" philosophy of Henry Carey and his circle. Careyite political economy envisioned a harmony between producers and consumers in which value emerged from their mutual enterprise. Instead of a limited specie money, liable to collect in foreign vaults, Careyites advocated a flexible, "nonexportable" legal tender currency.⁴¹ No explicit link between Careyite economics and racial equality exists; Careyite republicans also formed exclusionary associations based on race and gender. But the idea of value created in social cooperation often lent itself to the ideals of civil rights. Many of the most radical Republicans—Wendell Phillips, Thaddeus Stevens, William D. Kelley, and Benjamin Butler, for example—also supported greenbacks.

From the greenback perspective, a legal declaration of equality, an expression of political or cultural authority, could bring about equality just as a congressional declaration of the value of paper money could give the paper value. "After all, being a citizen and a voter has more than anything else made the negro a man," recalled Samuel Chapman Armstrong; "the recognition of his manhood has done much to create it."⁴² "All that is necessary for a government to do to create money," insisted a "soft money" Congressman, "is to stamp upon what it would change into money 'its image and superscription,' and it will be money."⁴³ The Fourteenth Amendment, declared Mississippi Republican James Alcorn, "created the being whom you now call a citizen of the United States" with one stroke of the pen. Faced with bankruptcy in Reconstruction Mississippi, Alcorn issued "certificates of indebtedness" to supplement the currency. This "Alcorn money," like the declaration of citizenship his government was based on, derived its value from political authority alone.⁴⁴

³⁸ Edward A. Pollard, *The Lost Cause Regained* (New York, 1868), 112–13.

³⁹ McCulloch, *Men and Measures*, 514.

⁴⁰ James S. Pike, *The Prostrate State: South Carolina under Negro Government* (New York, 1874), 82–83, 35–36.

⁴¹ On Carey's influence, see Unger, *Greenback Era*, 50–58; Sharkey, *Money, Class, and Party*, 153–56; Walter T. K. Nugent, *Money and American Society, 1865–1880* (New York, 1968); Nugent, *The Money Question during Reconstruction* (New York, 1967); and Rodney J. Morrison, "Henry C. Carey and American Economic Development," in *Transactions of the American Philosophical Society*, vol. 76 (Philadelphia, 1986).

⁴² Armstrong reprinted in Walter L. Fleming, ed., *A Documentary History of Reconstruction* (Cleveland, Ohio, 1907), 2: 434–35.

⁴³ *Congressional Record*, 43d Congress, 1st session (January 31, 1874): 1089.

⁴⁴ William C. Harris, *The Day of the Carpetbagger: Republican Reconstruction in Mississippi* (Baton Rouge, La., 1979), 291–94; *Congressional Record*, 43d Congress, 1st session, Appendix (May 22, 1874): 3.

Most often, the connection between money and racial identity appears in these sorts of parallel analogies. For example, the Albany, New York, *Atlas and Argus* expressed its disgust at the prospect of Negro soldiers by linking the Union's financial and military strategies. "For finance, issue Greenbacks," the editor wrote, "for war, Blackbacks."⁴⁵ Linton Stephens, brother of Confederate Vice-President Alexander Stephens, called the Fifteenth Amendment "exactly analogous to the prohibition on the States in the original Constitution—that no State shall coin money; emit bills of credit; make anything but gold or silver coin a legal tender." "Who ever dreamed," he continued, "that these prohibitions on the States gave Congress the power to control the whole subject of money?" By overstepping Constitutional limits, the Fifteenth Amendment thus "coined" counterfeit citizens.⁴⁶ The bullionist *Nation*, which under E. L. Godkin ferociously attacked both African-American equality and greenbacks during the 1870s, deplored Wendell Phillips' proposal that the Republicans nominate an African American for vice-president. Mr. Phillips believes, sneered Godkin, "that as soon as people saw a negro foisted into the Vice Presidency by political maneuvering, the whites would begin to respect the colored population more than they had previously." Phillips "might as well talk about regulating the temperature by forcing the mercury up and down in a thermometrical tube." Election, Godkin insisted, stemmed from popular recognition of character—it did not call that character into being. Godkin and Phillips echoed the terms of the money debate, asking if character inhered in certain objects and certain people or if value and character derived from social or cultural authority.⁴⁷

The crash of 1873–1874 highlighted these two points of view. As the crisis worsened, Congress engaged in an extraordinarily long exploration of the nature of money and value. Had a shortage of specie, fostered by eastern bankers and their European allies, caused the depression? Or had an uncertain, inflated economy, its morals degraded by irredeemable legal tender, collapsed under the weight of its own paper? Both the House and the Senate were also discussing Charles Sumner's Civil Rights Bill, which abolished most formal racial segregation. A review of congressional debate during this crisis offers another example of the congruence of money and race.

Resumptionists, wanting a return to specie, insisted that money must have intrinsic value. At times, they became overwrought, like New York Senator Jacob Cox. "Preciousness, cohesiveness and divisibility belong to gold as to no other element," Cox preached, "God has hardened it in the millions of years in which the mountains come and go like the rainbow. It is as true as its burnished source, the sun. Its silent power, like that of the dial, measures our height of prosperity or our depth of adversity."⁴⁸ Cox also favored a return to white rule: "the South," he wrote to James Garfield, "can only be governed through the part of the

⁴⁵ *Atlas and Argus*, January 19, 1863, quoted in Forrest G. Wood, *The Black Scare: The Racist Response to Emancipation and Reconstruction* (Berkeley, Calif., 1968), 44. Thanks to Alice Fahs for this reference.

⁴⁶ Linton Stephens reprinted in Alexander H. Stephens, *The Reviewers Reviewed* (New York, 1872), 245–46.

⁴⁷ *Nation*, 5 (August 1, 1867): 90. For the *Nation's* bullionist views, see below.

⁴⁸ *Congressional Record*, 43d Congress, 1st session (April 7, 1874): 2880.

community that *embodies* the intelligence and the *capital*" (my italics).⁴⁹ Cox made the link between gold and whiteness explicit, but most gold bugs simply insisted that without a common standard, difference lost all meaning, following Joseph Story's *Commentaries on the Constitution*: "Money is the universal medium or common standard, by a comparison of which the value of all merchandise may be ascertained, or, it is a sign which represents the respective value of all commodities."⁵⁰ Quoting Story, Chase argued in the second legal tender cases that, without such a standard, exchange "would become impossible," and we would lapse into barbarism. By this, he meant a state in which the meaning of the difference between goods and people was impossible to determine. Because of their special intrinsic value, "the selection . . . of gold and silver as the standard of value was natural, or more correctly speaking, inevitable" in advanced societies.⁵¹

James A. Garfield warned of "immutable laws of nature which no Congress can safely ignore and which no legislation can safely overturn." Three elements entered into exchange, he claimed: "extension, weight and value." The Constitution let Congress fix these standards, "but Congress cannot create extension, nor weight, nor value. It can measure what exists, but it cannot make length of that which has no length, it cannot make weight of that which has no weight, it cannot make value of that which has no value." Since all civilized nations of the world recognized gold, "let us restore and preserve our standard of value, which must be applied to every exchange of property between man and man." Congress could not legislate value or substance into being, nor could it derange the natural standard that gave meaning to difference.⁵²

August Merrimon of North Carolina believed that "money implies essential value. It is of the essence that money shall have essential value in it; and the experience of the world from the earliest periods down to the present time is that nothing constitutes the medium of exchange like gold and silver . . . It would seem that the Almighty had provided these substances to answer for this very purpose." Congress could pass a law tomorrow outlawing gold as money, he continued, and "it would still have value in spite of the statute."⁵³ Similarly, no one could legislate away racial difference. "Why did God make our skins white?" Merrimon asked in a speech on civil rights a few months later. "Why did He make the negro's skin black? Why did He make the other races of different color?" These natural differences gave evidence of God's design, he insisted, and "I ask Senators, where do they get the authority to change the color God has blessed us with? Where can any authority be found to make my skin black and corrupt my blood?"⁵⁴

Diluting the money supply diluted the nation's blood, and elevating the freedmen depreciated the value of whiteness. Responding to charges that contracting the currency by eliminating greenbacks was like bleeding a sick patient, Congressman Platt of Virginia asked, "Did the gentleman ever hear of a physician proposing to open the veins of a debilitated patient, and inject a large

⁴⁹ Quoted in Foner, *Reconstruction*, 499.

⁵⁰ Joseph Story, *Commentaries on the Constitution of the United States* [1833] (Boston, 1873), 2: 54.

⁵¹ *Knox vs. Lee, Parker vs. Davis*, 12 Wallace 583.

⁵² *Congressional Record*, 43d Congress, 1st session (April 9, 1874): 2973.

⁵³ *Congressional Record*, 43d Congress, 1st session (January 21, 1874): 801.

⁵⁴ *Congressional Record*, 43d Congress, 1st session (May 22, 1874), Appendix 316.

quantity of water therein?"⁵⁵ In debate on the Civil Rights Bill, Merrimon repeated the common claim that "intermingling" the blood of black and white led to degeneracy: "each side is degraded until the race becomes extinct." The search for intrinsic value—for a gold that always stays valuable—paralleled the search for racial purity and stable difference. When Merrimon talked about gold's intrinsic value, he suggested that gold locked in a vault for one's children would have the same purity and value one hundred years later, just as a carefully monitored bloodline would ensure children's intrinsic superiority. Racists believed in "hybrid degeneracy" despite physical evidence that miscegenous relations were "renegotiating" the meaning of racial difference, that "race" was not a stable category. In the same way, arguments about the "intrinsic value" of gold allowed hard money partisans to ignore gold's constant market fluctuations. Both depended on fantasies of purity and an escape from the constant renegotiations of the capitalist marketplace.

The instability of both race and money that Merrimon wanted to escape appears in one of David Locke's distasteful "Petroleum V. Nasby" satires of 1867. The story described a projected "Southern University." Among the exam questions proposed for the college was the following:

A high toned, shivilrous Virginian, twenty years ago, hed a female slave which wuz ez black ez a crow, and worth only \$800. Her progeny wuz only half ez black ez a crow, and her female grandchildren wuz sufficiently bleached to sell in Noo Orleans for \$2500 per female offspring. Required. 1st.: The length of time necessary to pay off the Nashnel debt by this means. 2nd.: The length of time required to bleach the cuss of color out of the niggers of the United States.

The story's "humor" came from linking race and money in ways that pointed to the negotiability of each. It suggested, first, that race tended to disappear in commercial intercourse—the term being chosen deliberately—and, second, that the erasure of racial difference might change the nation's stock of value and money. It echoed Merrimon's fear that intermarriage equaled inflation and the collapse of value.⁵⁶

This interpretation makes more sense compared to the greenback or expansionist position on money and citizenship. It seemed clear to many opponents of specie "resumption" that "an act of Congress declares what is money. It stamps the money quality, the representative value, upon a piece of gold or silver just as it does upon paper."⁵⁷ Greenbackers took comfort from the majority opinion of Justice Strong in the legal tender cases that "it is hardly correct to speak of a standard of value . . . [V]alue is an ideal thing."⁵⁸ Congressman David Mellish found "an inconsistency in attempting to make a standard of value" that itself had value all over the world. "The conundrum not yet solved," he suggested, "is how to measure the value of the measure of values. I venture to predict that this

⁵⁵ *Congressional Record*, 43d Congress, 1st session (April 7, 1874): 2871.

⁵⁶ *Petroleum V. Nasby* [David R. Locke], *The Struggles (Social, Financial, and Political) of Petroleum V. Nasby* (Boston, 1888), 398. Despite the satire, "Nasby" clearly agreed with Merrimon's position by making the idea of racial renegotiation laughable.

⁵⁷ Charles Francis Adams, "The Currency Debate of 1873–1874," *North American Review*, 119 (July 1874): 132.

⁵⁸ 12 Wallace 553.

difficulty will never be cured until something of no intrinsic value shall be adopted, and its value fixed conventionally or arbitrarily by a competent authority."⁵⁹

Greenbackers formulated a theory of value and identity drawn from nationalism and patriotic labor. "As surely as our flag represents . . . the unity of these States," declared a Michigan greenbacker, "just so surely, sir, do the United States Treasury notes represent the cost of life and blood and treasure, the priceless value of that unity of States." The notes' value came from cooperative work and sacrifice. But, by lowering interest rates and aiding debtors, by stimulating the economy, greenbacks supposedly helped create the same egalitarian economic conditions they symbolized. Just as each citizen required political representation, each citizen required a physical representation of his value. Massachusetts Radical George Boutwell, for example, suggested that the presence of four million new citizens, each eager to work, spend, and own property, required more currency; otherwise, a shortage of money might deprive the freedmen of the building blocks of citizenship. Boutwell's point was clear—having created new citizens, the United States must create the money necessary to represent them in commerce.⁶⁰

New Yorker Clinton Merriam disdained "the late Confederate currency, based upon a triumph of aristocratic ideals," and praised the republican greenbacks that won the war. Greenbacks had made citizens of the ex-slaves. "The greenback was the first thing they ever earned that they could call their own, the first thing, save our flag, that stood before them, a symbol of their freedom." Greenbacks symbolized the power of government to overturn the natural law arguments that justified slavery. "With it they soon learned a power to gather together long-broken families into a common home," Merriam continued. "To the colored men the greenback rose above the dignity of language; to them it almost bore the dignity of religion." Greenbacks made it possible for slaves to own property and establish families, the two bulwarks of republican citizenship. It also taught them the religion of saving and self-advancement.⁶¹

Merriam described African Americans engaged in "self-making," approaching equality by prospering and thereby forcing the nation to reconsider what racial difference meant. He referred his colleagues to the Freedmen's Savings Bank, in which millions of those "precious-earned greenbacks" resided. By learning to work and save, "they are progressing to a better manhood . . . , becoming useful citizens of our republic. All this was accomplished from the honest sweat of honest brows while using our 'irredeemable, depreciated trash,'" he concluded sarcastically. The greenbacks helped create the value and identity they represented.⁶²

But could citizens created with the help of valueless currency ever acquire any genuine value of their own? The Freedmen's Savings Bank had been founded in

⁵⁹ *Congressional Record*, 43d Congress, 1st session (January 31, 1874): 1099. Two months after this speech, Mellish died in an insane asylum. "He devoted himself almost exclusively to the study of the currency," noted Garfield, "became fully entangled with the theories of the subject and became insane." That other colleagues drew the same conclusion discloses a key to the nature of the money debate. Mellish also supported civil rights. Garfield quoted in Allen Weinstein, *Prelude to Populism: Origins of the Silver Issue, 1867–1878* (New Haven, Conn., 1970), 244.

⁶⁰ *Congressional Record*, 43d Congress, 1st session (April 9, 1874): 2967. See also March 10, 1874: 2099. Boutwell cited in Sharkey, *Money, Class, and Party*, 71.

⁶¹ *Congressional Record*, 43d Congress, 1st session (March 27, 1874): 2546.

⁶² *Ibid.*, 2545.

an attempt to "republicanize" ex-slaves by teaching them the proper relationship of money and value. Work, save, invest, and watch your money grow, they were told, and attain equality. But could they ever measure up to the standard of *homo economicus*? George Fitzhugh doubted it. "Political economy stands perplexed and baffled in the presence of the negro. Capital can get no hold on him" because the Negro lacked the natural attributes. "We must quite expel nature before we can make the negros the equals of the whites."⁶³

The Freedmen's Bank collapsed, thanks to undercapitalization and mismanagement rather than African-American improvidence.⁶⁴ But some critics, hostile to racial equality, linked the bank's failure to both the character of its depositors and the economic immorality of Reconstruction. Like the "wildcats" that preceded it, the bank spent far more money on investments than it could ever hope to pay its depositors. Dealing in greenbacks, it supposedly lacked the natural regulation and restraint intrinsic to a specie economy. A reporter in Savannah, Georgia, observing the Freedmen's Bank, sniffed that "the modern savings bank is little more than a lottery, and has too many of the worst features of the lottery, as it was managed by northern men in the Southern States." But lotteries suited African Americans, he thought. The "same natural impulse" that made the freedman especially fond of the lottery "finds him with his little savings and his rags at the counter of the Freedman's Savings-Bank . . . to deposit his money in the happy belief that [the bank] will make it grow into a fortune." The comment at once deplored the African-American depositor, his "rag" paper money, the bank he put that money in, and the political economy that made all three possible.⁶⁵

The phrase "savings and rags" recalls Thomas Nast's caricature of inflation and paper money as "the rag baby." A famous Nast cartoon of 1876 depicted a rag doll slumped before a printed handbill (Figure 1). "This is not a rag baby but a real baby, by act of Congress," insisted the note, while a disembodied hand offered the rag doll a slip of paper reading "this is MILK by act of Con." Nearby, an outline drawing of a cow captioned "this is a cow by the act of the—artist," and a crudely fashioned dollar bill labeled "this is money by the act of Congress" further spoofed the possibility of simply declaring real people and things to be something they were not. By 1876, Nast had abandoned his earlier depictions of the freedmen as noble, deserving citizens and instead drew them as degraded, ape-like semi-savages. Northern sentiment likewise began moving away from racial equality and toward notions of intrinsic racial hierarchy. Nast's characterization of paper money as a wishful fantasy became more persuasive as his depictions of intrinsic racial difference grew more pronounced.⁶⁶

Nast's rag baby cartoon appeared in *Robinson Crusoe's Money*, a gold standard fable by economist David A. Wells based on that hoary staple, the Robinson

⁶³ Quoted in Fabian, *Card Sharps, Dream Books, and Bucket Shops*, 127.

⁶⁴ Carl R. Osthaus, *Freedmen, Philanthropy, and Fraud: A History of the Freedmen's Savings Bank* (Urbana, Ill., 1976); Walter Lynwood Fleming, *The Freedmen's Savings Bank: A Chapter in the Economic History of the Negro Race* (Chapel Hill, N.C., 1927).

⁶⁵ Quoted in Fabian, *Card Sharps, Dream Books, and Bucket Shops*, 135–36. I am greatly indebted to Professor Fabian's provocative discussion of the Freedmen's Bank.

⁶⁶ Foner, *Reconstruction*, gives a good example of Nast's changed vision. See illustrations facing page 386.



FIGURE 1: Thomas Nast, "Milk Tickets for Babies, in Place of Milk." From David A. Wells, *Robinson Crusoe's Money* (1876).

Crusoe example.⁶⁷ Like many of his contemporaries—Social Darwinist William Graham Sumner, for example, was a fierce gold bug—Wells connected political

⁶⁷ David A. Wells, *Robinson Crusoe's Money*; or, *The Remarkable Financial Fortunes and Misfortunes of a Remote Island Community* (New York, 1876), 97.

economy to evolution. Rejecting gold as the standard of value, he believed, "makes warfare upon the beneficence of the Almighty," violates "the natural laws of supply and demand," and attempts "to provide for survival of the unfittest."⁶⁸ Political economy texts of the time routinely imagined a progression in money, in which primitive peoples used wood or shells, more advanced societies used base metals, until, finally, gold emerged as the common standard of the Anglo-Saxon and Teutonic peoples. Connections of specie to progress occur again and again, always linking primitive "races" to valueless money and gold to the most advanced.⁶⁹

In *Robinson Crusoe's Money*, Wells described the virtues of gold in ways that strongly recalled popular anxieties about racial purity. At first, Crusoe is alone and would, "had he remained alone on the island, have become a pure savage, and simply because he was alone and could make no exchanges."⁷⁰ Wells equated exchange and difference with civilization, sameness with savagery. Soon, others arrived, however, and a period of barter began. Eventually, the racially mixed islanders turned to gold when they noticed that "an admixture with it [gold] of the slightest impurity or alloy immediately changed its color" and that "color became to a very high degree a test of its purity." They also noticed that "every piece, no matter how small, possessed the attributes of every other larger piece, and that when any larger piece was divided into a great number of smaller pieces, these last, in turn, could be reunited without loss or difficulty again into one whole." Adopting gold as their standard of exchange, they "determined to be an honest and free people" with specializations of labor based on intrinsic ability.⁷¹ The description of gold's properties, while empirically true, reconnected gold to the language of racial purity and social hierarchy. Nast's cartoon for the cover of *Robinson Crusoe's Money* depicted a gold coin with the head, feet, and tail of a rooster, crowing atop a rubbish heap composed of law books, "bluebacks," acts of Congress, and decisions of the Supreme Court (Figure 2). The caption proclaimed "the survival of the fittest."⁷² The cartoon joins Wells's text in linking arguments for gold to a Social Darwinist account of social hierarchy.

Despite the "elective affinity" between "specie" and "species," it would be as much a mistake uniformly to charge the gold bugs with racism as it would automatically to equate the greenback position with equality. A hard money man such as Charles Sumner could easily support racial equality: Sumner believed that

⁶⁸ David A. Wells, *Practical Economics* (New York, 1885), 52–53.

⁶⁹ For good examples, see Francis A. Walker, *Money* (New York, 1883), 31–43; and *Political Economy* (New York, 1887), 122–27; Nugent, *Money and American Society*, 89–90. Greenbackers also used the same progression, arguing that legal tender represented its next logical step. See William A. Berkey, *The Money Question: The Legal Tender Paper Monetary System of the United States* (Grand Rapids, Mich., 1876), 27–28.

⁷⁰ Wells, *Robinson Crusoe's Money*, 14.

⁷¹ Wells, *Robinson Crusoe's Money*, 38–39. "Sameness" is obviously a major problem in market capitalism. Political economists such as Wells routinely described savages as degraded by their inability to make distinctions, a notion derived from Adam Smith's insistence on linking progress to the division of labor. See Chase's comments above, n. 51. Sameness marks the collapse of civilization in racist fantasy in the same way that horror at a mongrel population, uniformly undistinguished, characterizes a great deal of anti-miscegenation literature. But sameness also attracts racists, in that only people who are essentially "the same" can make a race. William Faulkner's novel *Absalom, Absalom!* (1936) treats this paradox at length.

⁷² Wells, *Robinson Crusoe's Money*, 52. See also Walter Benn Michaels' description of Wells's book in *Gold Standard and the Logic of Naturalism*, 145–50.



FIGURE 2: Thomas Nast, "The Survival of the Fittest." From David A. Wells, *Robinson Crusoe's Money* (1876).

there were intrinsic qualities necessary to citizenship, and African Americans possessed them. Humanity itself, a shared nature, could appear as a "gold standard" that linked all men and women. African Americans struggling for equality found this "intrinsic value" position more useful than the claim that

equality came from legislative enactment alone. Frederick Douglass himself described the main branch of the Freedmen's Bank as a sham, all appearance and no real value: "when I came to Washington and saw its magnificent brown stone front, its towering height, its perfect appointments and the fine display it made . . . I felt like the Queen of Sheba when she saw the riches of Solomon, that 'the half had not been told me.'" And, indeed it had not, as Douglass soon discovered—the riches of the bank turned out to be every bit as fabled as Solomon's, "scarcely more substantial than a dream."⁷³ A people struggling toward equality needed a more tangible foundation than dreams, and greenbacks, for their efforts.

African Americans during Reconstruction clearly believed that their claim to equality came from both the possession of intrinsic rights and the value created by their labor. Congressman Robert B. Elliot insisted in debate over Sumner's Civil Rights Bill that African Americans enjoyed the God-given "natural rights" common to all men. He further recalled how his people's toil, in the fields and armies of the republic, had earned them a place as equal citizens. African Americans served in the revolutionary war and with Jackson in New Orleans: they "flew willingly and gallantly to the support of the national Government during the Civil War . . . Their sufferings, assistance, privations, and trials in the swamps and in the rice fields, their valor on the land and on the sea, is a part of the ever-glorious record which makes up the history of a nation preserved."⁷⁴ The rights elaborated in the Declaration of Independence, Congressman Richard H. Cain contended in the same debate, "are our rights by inheritance, and by the inexorable decree of Almighty God." But he also reminded his white colleagues that "we have toiled with you in building up this nation," "we have suffered side by side with you in the war," and that "inasmuch as we have together passed through affliction and pestilence let there now be a fulfillment of the sublime thought of our fathers—let all men enjoy equal liberty and equal rights."⁷⁵ African Americans in Congress recognized that the greenback position offered no sureties. For many greenbackers, the legal declaration of equality only legitimated a laissez-faire attitude about race relations, in that it established a democratic playing field free of artificial restrictions on the money supply.

Nor did soft money politics preclude racism. Facing a sometimes chronic shortage of specie, many southern Democrats endorsed greenbacks while holding firmly to a racial hierarchy. "Throughout the 1870s it was generally agreed in the South that the region's supply of money was desperately deficient . . . as a result, southern congressmen pressed unceasingly for an increase in the money supply, and they voted solidly against contraction and specie resumption."⁷⁶ It seems perfectly reasonable, from the logic of the argument presented here, that greenbackism might look more plausible in a society committed to racial inequality: the presence of persons clearly and non-negotiably different could make a certain degree of economic indeterminacy tolerable. For example, the "free banking" era described above, in which thousands of different kinds of currency

⁷³ Frederick Douglass's *Life and Times* [1881] quoted in Fleming, *Freedmen's Savings Bank*, 88, 89.

⁷⁴ *Congressional Record*, 43d Congress, 1st session (January 6, 1874): 410.

⁷⁵ *Congressional Record*, 43d Congress, 1st session (January 10, 1874): 566.

⁷⁶ Michael Perman, *The Road to Redemption: Southern Politics, 1869–1879* (Chapel Hill, N.C., 1984), 271–72.

circulated, might only have been tolerable in a society containing racial slavery.⁷⁷ Greenbackers often tended to be more sanguine about racial equality, given their origins in Carey's producerist, small capitalist version of the labor theory of value. Greenback philosophy suggested that African Americans could earn their place in a society of producers, while gold bug essentialism either granted them this status or denied it. In general, the greenback position may have offered a more optimistic model of racial coexistence, as the Populist Party, for all the debate over its sincerity, suggested when it unsuccessfully reconnected racial equality and greenback dollars in the 1890s.⁷⁸ Even if no entirely consistent link exists between monetary policy and attitudes toward racial equality, the discursive similarity between the language of race and the language of money—the peculiar fetishizing of both intrinsic value and racial purity—seems more than coincidental.

The point here, again, is not to map the connection between individuals and interests but rather to offer a broader examination of how those interests were conceived and to explore a paradox that seems central to free market liberalism itself. The two words, "individual," and "interest," both arise from the same political economy. Imagining a rational, profit-maximizing individual requires imagining rational interests for him to maximize. The similarity between the dual essentialisms of race and money tends to collapse this distinction, since the stability of both "race" and "money" often depended on fantasies of intrinsicity that mirrored each other. Free market liberalism, as described here, is not rational at all. If liberal ideology extolled the individual's capacity for unlimited self-making, it also at the same time and even in the same persons argued for an essential nature removed from market negotiation. Paradoxes are enormously creative; attempts to resolve them generate energy, ideas, and innovations. But we can hardly expect a coherent connection between interests and actions from a political philosophy that continually subverted its own best ideals.

As it became clear that African Americans wanted more than a permanent place on the bottom rail of the free market, the greenback position became increasingly unsettling and untenable. Is it mere coincidence that when ex-Confederates began overturning carpetbag rule, they called themselves "redeemers"—the same word used to describe the process of turning paper money in for gold, or "redeeming" the greenbacks?⁷⁹ Connections between gold and race

⁷⁷ But not entirely tolerable. See, for example, Jackson's message vetoing the Second United States Bank's recharter, which claimed that legislation was powerless to abolish "real" or intrinsic natural distinctions, whereas eliminating the bank and its irredeemable paper trash promised to restore a natural political economy. Jackson equated paper money, and the social creation of value, with artificial social distinctions. He linked the gold standard and a specie economy to natural law meritocracy—with himself "naturally" at the head. See "Message by President Andrew Jackson Vetoing the Bank Recharter, July 10 1832," reprinted in Krooss, *Documentary History of Banking and Currency*, 2: 36.

⁷⁸ The extensive historiographical debate over the Populists' commitment to racial equality is summarized in Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976); and Bruce Palmer, *"Man over Money": The Southern Populist Critique of American Capitalism* (Chapel Hill, N.C., 1980), and need not be repeated here. Whatever the intensity of their commitment to equality, no political party of the time came even remotely close to matching the record of the those Populists most closely tied to greenback ideology.

⁷⁹ There is a certain consistency between the two meanings of "redeem," especially given the origins of "carpetbagger" in paper money and wildcat banking. If carpetbaggers circulated irredeemable paper notes, "redeemers" restored the proper relation of notes to specie. "Redeemers"

became less suggestive and more explicit as Reconstruction waned. When Charles Francis Adams saw that many white Southerners supported greenbacks, he connected their stance to the decline in their morals brought about by Reconstruction misrule. "Crushed under negro legislation," he bitterly declared, why should the South "care to maintain those standards of value which represent only ruin to themselves?" Their moral economy deranged by African-American equality, white Southerners' political economy collapsed as well.⁸⁰ By the end of the 1870s, hard money southern Democrats had come to share Adams's view.

In the North, liberal Republicans such as Adams led the flight from equality, often linking inflated bills to inflated citizens. "The inflation ring," the *Nation* charged, always disguises its economic corruption "with some philanthropic work, such as the care of the poor or the elevation of the black man." Commenting on political corruption and deficit spending in South Carolina, Godkin called the greenback position a disease, "the same disease [that] has been rotting away the fibre of the South Carolina government," producing "a swarm of little Tweeds and little Butlers, some black and some white."⁸¹ He connected Boss Tweed's corruption of immigrants with inflated dollars to Benjamin Butler's advocacy of greenbacks and racial equality. Only a hard money standard could restore moral and racial order.

Liberal Republicans such as Godkin, who advanced the intrinsic value of gold against an inflated political economy, also began searching for intrinsic values in art and literature. Lawrence Levine has described the "emergence of cultural hierarchy" after the Civil War, the idealization of a body of literary and artistic works whose merit supposedly transcended both history and the market. Appalled by "pseudo culture," by a "chromo-civilization" that mass-produced cheap reproductions of great art, genteel critics made a cult of the original, the difficult, the refined and pure. In this view, great works of art such as Shakespeare's plays possessed an intrinsic, timeless, universal value that endured through all attempts at quantification or sale to the masses. Typically, acolytes of "high culture" presented these works as pure expressions of a racial or quasi-racial heritage—all that is best in the Western tradition—much as, today, advocates of a literary canon insist on the intrinsic, apolitical, and timeless value of certain texts. The very word "highbrow," used to describe the lofty activities of elite culture, came from attempts to establish racial differences in skull shape and size: European high brows against ostensibly low-browed Africans and Asians. In a Social Darwinist context, the highbrow's ownership of great art demonstrated intrinsic qualities of superiority and leadership. As Andrew Carnegie wrote, "It is well, nay, essential, for the progress of the race that the houses of some should be homes for all that is highest and best in literature and the arts, and for all the refinements of

not only overturned carpetbag rule, they also reversed expansionist and inflationary policies. But at this point, this connection is purely suggestive.

⁸⁰ C. Adams, "Currency Debate of 1873-1874," 117.

⁸¹ *Nation*, 18 (April 16, 1874): 246, 247. On the liberal Republicans and their rejection of racial equality, see Foner, *Reconstruction*, 488-511, 526-29; and John G. Sproat, *The Best Men: Liberal Reformers in the Gilded Age* (New York, 1968), 11-44.

civilization.”⁸² Facing an unsettling challenge to the stability of value and identity, the genteel elite found comfort in acquiring “priceless” works of art that reflected a timeless, intrinsically valuable racial character.⁸³

The clearest link between inflated value and racial equality appears in the novel that gave the period its name, *The Gilded Age*, by Mark Twain and Charles Dudley Warner. The plot centers on the attempt by Colonel Beriah Sellers to fund a southern “Industrial University”—not unlike the Southern College satirized above by Petroleum V. Nasby—“open to all persons without distinction of sex, color or religion.” The action takes place during Reconstruction, and the “Tennessee Knobs Industrial University” parallels Republican attempts to reform southern education. Sellers represents the inflationist position, the cultural creation of value. Eternally, wildly optimistic, Sellers wants a currency based on everything, including pork: “What we want is more money . . . [B]ase it on everything!” Echoing the congressional declarations in Nast’s cartoon, Sellers tries to transmute reality with mere words. “The Colonel’s tongue was a magician’s wand that turned apples into figs and water into wine as easily as it could change a hovel into a palace and present poverty into imminent future riches.” By connecting Sellers’s puffery to the Industrial University—making the ideal of integrated education the heart of their novel—Twain and Warner connected the enterprise of racial and gender equality to inflated value.⁸⁴

THE CONNECTION BETWEEN RACE AND MONEY described here suggests that the freer a market society becomes, the more it would imagine differences—such as racial or sexual difference—that resist negotiation because of their “intrinsic” character. American political economy gives clear examples of this tendency toward essentialism from its origins, as Edmund S. Morgan pointed out.⁸⁵ The Constitution upheld the sanctity of intrinsic difference when it legitimated racial slavery. Slaves could never be “self-made,” could never erase racial difference and make themselves over into white people. “The prejudice rejecting the Negroes,” Toqueville noticed, “seems to increase in proportion to their emancipation.” In other words, the freer they are, the more they are imagined as intrinsically different, and, as he also predicted, “the freer the whites in America are, the more they will seek to isolate themselves.”⁸⁶ Liberal ideals accommodated both a belief in inherent, fundamental difference between free individuals and groups and a belief in the possibility of erasing that difference. But the more real the possibility of erasure became, the more fundamental difference seemed to appear. Reconstruction marked one instance of how African Americans’ assertion of equality intensified theories of racial difference; the 1890s, discussed briefly below, offer another. But

⁸² Carnegie quoted in Nell Irvin Painter, *Standing at Armageddon: The United States, 1877–1919* (New York, 1987), 94.

⁸³ Lawrence W. Levine, *Highbrow, Lowbrow: The Emergence of Cultural Hierarchy in America* (Cambridge, Mass., 1988), esp. 219–31.

⁸⁴ Mark Twain and Charles Dudley Warner, *The Gilded Age: A Tale of Today* [1915] (New York, 1969), 333, 59.

⁸⁵ Morgan, *American Slavery, American Freedom*.

⁸⁶ Alexis de Tocqueville, *Democracy in America*, J. P. Mayer, ed. (New York, 1988), 344, 356.

this paradox, this tension between malleable and fixed character, has always been the dominant problem in American thinking about money, value, and markets.⁸⁷

Markets exist in large part to put a value on difference—different grades of food, different purities of gold, different kinds of work—in other words, to assess the meaning of difference. Markets serve many other purposes: distributing goods, playing politics, allowing social interactions of all kinds. But a market depends on exchanges, on giving meaning to difference. If I have two bushels of corn, and you have two of wheat, we recognize a difference. If we meet to exchange our wheat and corn, we need to assess the meaning of the difference between the two, the relative value of each. Market exchanges often depend on finding differences between things that seem largely alike—two different shirts, for example, or two kinds of apples. Exchanges tell us if spotted apples are better (or worse) than smooth apples by setting a value on each. So, by its very nature, a market constantly finds “difference” and simultaneously finds ways to give that difference a fixed, stable meaning.⁸⁸ Most often, markets establish this meaning in terms of price and symbolize it in money. Money, then, is the sign of the meaning of difference.⁸⁹

To allow the constant negotiation that gives meaning to difference—and to self-evident truths—money itself must be stable. Money mediates between social categories, between different kinds of goods linked only by someone's desire to sell them. Moving between the categories of self and other, dirty and clean, sacred and profane, money reinforces them. Money signifies difference because it negotiates social boundaries. That is, an item cannot become salable until its specific, concrete properties are known well enough to imagine an equivalent. By moving between two different goods, money helps shore up the difference. But if the money becomes unstable, so do the boundaries it helps define.⁹⁰ Giving the differences between people and things, signified by price, any real, stable meaning requires that the thing that signifies difference—money—have fixed meaning itself.

⁸⁷ See, for example, Jean-Christophe Agnew, *Worlds Apart: The Market and Theater in Anglo-American Thought, 1550–1750* (Cambridge, 1986); Rogin, *Fathers and Children*, chap. 3; and Michael Paul Rogin, *Subversive Genealogy: The Politics and Art of Herman Melville* (New York, 1983); Halttunen, *Confidence Men and Painted Women*; Susan Gillman, *Dark Twins: Imposture and Identity in Mark Twain's America* (Chicago, 1989); Gillian Brown, *Domestic Individualism: Imagining Self in Nineteenth-Century America* (Berkeley, Calif., 1990).

⁸⁸ See, for example, Adam Smith's founding distinction between “real” and “nominal” price in *The Wealth of Nations*, or his insistence in the same book that the market, by creating a diversity of occupations, reveals a fundamental or essential character, an innate and intrinsic ability. Claims that the market produces efficiencies are often claims that the market is revealing essential character or ability. Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations* [1776] (Oxford, 1976), 25–31.

⁸⁹ A roughly similar claim can be found in Marc Shell, *Money, Language, and Thought: Literary and Philosophical Economies from the Medieval to the Modern Era* (Berkeley, Calif., 1982).

⁹⁰ The work of Mary Douglas, especially Mary Douglas and Baron Isherwood, *The World of Goods* (New York, 1979), 59–70, is crucial here. In a society that does not comprehend a difference between, for example, sacred and profane, exchanging one for the other would be impossible; the difference simply does not exist and cannot be given meaning. But, once the difference is established, it can be given meaning easily by a price. Sacred things are expensive, even “priceless,” and profane things cheap or even “worthless.” Money can buy them both, but only if the difference between them is clearly understood and signifiable. See also Douglas, *Purity and Danger: An Analysis of the Concepts of Pollution and Taboo* (London, 1966); and Douglas, *Implicit Meanings: Essays in Anthropology* (London, 1975).

THIS ESSAY HAS DESCRIBED CONFLICTING IMPULSES at the heart of liberal individualism and their reoccurrence in simultaneous debates about race and money. The persistence, during Reconstruction, of the debate between essentialism and social convention reveals the roots of the American political economy in, and at the same time its uneasiness over, definitions of essential difference. Five years after the close of Reconstruction, the greenback position had moved from mainstream to margin, increasingly confined to a radical agrarian minority.⁹¹ As the North's always flimsy commitment to racial equality dissipated, the sham debate over gold and silver swept greenbacks from the field. Democrats and some Populists embraced the silver panacea in the 1890s because it offered them the inflationary and anti-monopoly rhetoric without the radical political content, the comfort of an intrinsic value position behind the sound and fury of William Jennings Bryan's charge.⁹²

Some previous explanations for the political triumph of specie have focused on interest-group politics.⁹³ Certainly, personal and class interest, however defined, played a major role in individual positions in the politics of the money supply, both during Reconstruction and after. Obviously, people did not choose paper or specie merely on the basis of a vaguely felt anxiety about identity.⁹⁴ But, as noted above, an interest-based analysis reproduces the separation between categories of knowledge this essay attempts to critique. Even more telling, "interest" fails to explain what gave the fetish of intrinsic value its hold on the imagination.⁹⁵ The

⁹¹ Greenback ideas persisted in the Greenback Labor and Greenback parties, but their electoral defeat (despite the large Greenback vote in 1878) marked them as marginal, especially compared to their strength during Reconstruction. The failure to build an urban alliance also indicated the relative weakness of greenbackism. Henry Demarest Lloyd's sympathetic account of the Populist Convention in 1896 treats the Greenback elements of the party as clearly the revival of a moribund tradition when he refers several times to former members of the "old" and "venerable" Greenback movement. Lloyd, "The Populists at St. Louis," reprinted in George B. Tindall, ed., *A Populist Reader: Selections from the Works of American Populist Leaders* (New York, 1966), 213, 217, 220. Lawrence Goodwyn gives a convincing account of the marginality of greenbackism for all but the Populist Party by the 1890s and of its inability to transcend the language of silver in *Democratic Promise*.

⁹² Richard Hofstadter pointed to the anti-Semitism of the free silver movement in *The Paranoid Style in American Politics* (New York, 1965) and in his introduction to William H. "Coin" Harvey, *Coin's Financial School* (1894; rpt. edn., Cambridge, Mass., 1963). While Hofstadter confused Populists with the free silver fringe, the point about free silver and anti-Semitism is well taken. Opponents of the gold standard linked gold accumulation to Jewish rapaciousness, a racial stereotype, and they called silver "the dollar of our daddies," or the natural mineral standard of the Anglo-Saxon race. Harvey consistently personified gold and silver, as in "[the act of 1873] deprived silver of its right to free coinage" (106) or "gold needs to be taught that lesson," and "gold must be given to understand this not indispensable" (226-27). Again, in this preposterous debate, the language of racial difference and the language of intrinsic specie value come together.

⁹³ See Unger, *Greenback Era*; Sharkey, *Money, Class, and Party*; and Nugent, *Money and American Society*. Unger argued that interest alone could not explain individual positions on the money question, then went on to try to map these interests anyway. He acknowledged, however, that ideology, in the form of religious or intellectual beliefs, could influence monetary policy. He attributed the gold fetish in part to the power of nineteenth-century science, and on this we agree—except that scholarship since the 1960s has made it impossible to ignore the racism pervading American science.

⁹⁴ See J. Mills Thornton III, "Fiscal Policy and the Failure of Reconstruction," in Kousser and McPherson, *Region, Race, and Reconstruction*, 349-416. Thornton connects the failure of Republican rule in the South to a gross mismatch between the taxes southern whites had to pay, in lieu of the old tax on slaves, and the state expenditures they received.

⁹⁵ For example, it is well known that hard money advocates wanted U.S. Bonds, bought during the Civil War with greenbacks, to be repaid in specie—a substantial gain. Thus the "high-sounding

answer is the market's energetic volatility itself, coupled to African Americans' equally energetic insistence on taking the promises of free market liberalism seriously. Market volatility, with its anxiety about fraud and false value, is the precondition; African-American enterprise is the cause. The possibility that African Americans might renegotiate the meaning of race intensified an irrational interest in essentialism, and this emphasis on the genuine, the intrinsic, and the "natural" made both greenbacks and African-American equality unthinkable.

This same anxiety intensified again in the 1890s when African Americans, following men such as Booker T. Washington, began reasserting their claims to economic self-determination. The height of the nineteenth-century gold and silver debate—1896—came as southern whites were establishing formal racial segregation and disfranchisement. By the 1890s, a generation of African Americans born in freedom had abandoned old patterns of racial deference. "The young negroes are too self assertive," claims a former planter in Charles Chesnutt's 1901 novel *The Marrow of Tradition*: "Education is spoiling them." He refers to a young nurse, trained in the local African-American school. Her professional identity threatens to make race irrelevant. Whites "gave her nothing but her wages," she felt, "and she owed them nothing more than equivalent service. It was purely a matter of business."⁹⁶ In the market, she might be not white or black but rather a nurse. The diversification of labor had given her a new identity, as Adam Smith and Booker T. Washington predicted. Facing the possibility of the market remaking the meaning of racial difference, Jim Crow laws fixed the nature of the difference between black and white, rendering it "non-negotiable." Chesnutt expressed this situation eloquently when he described his main character, a prosperous, refined, light-skinned African American trained in the North as a doctor who was forced by law to sit in a Jim Crow train car. He muses on the signs that enforce segregation:

The author of this piece of legislation had contrived, with an ingenuity worthy of a better cause, that not merely should the passengers be separated by the color line, but that the reason for this division should be kept constantly in mind. Lest a white man should forget he was white, . . . these cards would keep him constantly admonished of the fact; should a colored person endeavor, for a moment, to lose sight of his disability, these staring signs would remind him continually that between him and the rest of mankind not of his own color, there was by law a great gulf fixed.⁹⁷

argument" for intrinsic value conceals base self-interest and greed. But then the question remains: What gave the intrinsic value "cover" its appeal? What made it "high sounding"? Hofstadter, among others, critiqued the symbolic force of intrinsic-value arguments, characterizing the desire for specie money among silver neo-populists as "folkish" and primitive and connecting it to racial anti-Semitism. But the nation's leading businessmen and economists shared these fantasies of intrinsic value. Irwin Unger connected Jacksonian agrarian support for intrinsic-value arguments to lack of sophistication but linked gold bug arguments for the same thing to scientific education. If intrinsic-value arguments pervaded American culture, at both levels, then they cannot be explained merely by a specific intellectual and cultural milieu. They are closely linked to arguments about genetic or biological character, arguments that in turn legitimate and explain the positions that individuals and groups hold in the market. Richard Hofstadter, "*Coin's Financial School and the Mind of Coin Harvey*," Harvey, *Coin's Financial School*, 47; Unger, *Greenback Era*, 135–44.

⁹⁶ Charles W. Chesnutt, *The Marrow of Tradition* (1901; rpt. edn., Ann Arbor, Mich., 1985), 43, 42.

⁹⁷ Chesnutt, *Marrow of Tradition*, 56.

In the same year as the electoral defeat of free silver, the Supreme Court legitimated this arbitrary separation in *Plessy vs. Ferguson*. Homer Plessy, the plaintiff, was only one-eighth black; he looked to all intents and purposes like a white man. Attorney Albion Tourgée, who argued Plessy's case, hoped, by presenting the court with a white "black" man, to point up the incoherence and negotiability of racial categories. Instead, the court only affirmed them, implicitly accepting the famous "one drop rule"—that one drop of African blood was enough to color a whole ocean of Caucasian whiteness. "Legislation," claimed the majority opinion, "is powerless to eradicate racial instincts or to abolish distinctions based on physical difference."⁹⁸ Facing the possibility that men such as Plessy could renegotiate racial value—facing the "staring sign" of Plessy's mixed heritage—the court responded with irrational theories of intrinsic racial difference. The gold and silver debate was inseparable from this intense and pervasive desire to find certainty, stability, and genuineness in the face of market renegotiations of identity and position.

The money question needs to be reconsidered in the context of a widespread and deeply rooted anxiety about value and identity in Victorian American male culture. Late nineteenth-century arguments about money closely mimicked arguments about racial and gender identity. They also mirrored attempts to find and "canonize" a Western artistic and literary heritage. It is no accident that the "battle of the standards" took place when it did. The money question consumed American politics at the same time that Jim Crow laws swept the South, educated "New Women" challenged traditional male prerogatives, Eastern and Southern European immigration began to increase, and the mass production of consumer goods blurred the line between genuine art and mere imitation. Advocates of both gold and silver tried to contain the shifting sands of identity and value with a resort to nature, to supposedly inherent, timeless natural qualities—"timeless" qualities such as the superiority of the Anglo-Saxon race, the domestic instinct of the female, or the eternal genius of Shakespeare. The supposedly "inherent value" of gold or silver resisted adulteration in the same way that the almost invisible admixture of black blood in Homer Plessy's inheritance could never vanish, would make him always and forever a black man. No matter how much social, economic, or even sexual exchange went on, Plessy stayed black, women stayed happier in the home, and great art would remain figuratively if not literally "priceless." Instead of exploring the paradox of value and identity at the heart of liberal capitalism, Americans discussed which of the two "intrinsically valuable" metals best represented a society firmly committed to racial and gender inequality.

⁹⁸ 163 U.S. 537 (1896). My interpretation of *Plessy* draws substantially on Eric J. Sundquist, "Mark Twain and Homer Plessy," in Susan Gillman and Forrest Robinson, eds., *Mark Twain's Pudd'nhead Wilson: Race, Conflict, and Culture* (Durham, N.C., 1990).

Thinking about the Languages of Money and Race:
A Response to Michael O'Malley, "Specie and Species"

NELL IRVIN PAINTER

"SPECIE AND SPECIES" IS A SURPRISING AND PERCEPTIVE ESSAY that inquires into the shared discourse of money and race in mid to late nineteenth-century American thought. Looking less to events and political parties and more to structures of meaning as revealed in the use of language, Michael O'Malley discovers a new level of historical significance, signaled by linguistic patterns pervading American political economy. He looks to counterfeiting and the controversies surrounding fiat money and specie for concepts that reappeared in discourse about race during Reconstruction. These habits of thought, he says, manifest a fundamental tension between the ideals of freedom and the quest for fixed values during a time of economic and political change. I wish historians would do more of this kind of work.

The separate vocabularies of money and race are familiar, but, by juxtaposing their shared imagery, O'Malley finds ideological reasons for what he terms the failure of Reconstruction: when the economy was in flux and possibilities for individual mobility were open, Americans needed a place of certainty, and that place was the terrain of race. He finds that the market economy grew increasingly free coinstantaneously with both the hardening of rigid racial categories and an insistence that money be backed by specie.¹ Remarking on the fetishization of intrinsic worth and racial purity, O'Malley concludes that just as a majority of citizens opposed currency whose worth depended purely on the decree of the federal government, so this majority also opposed the creation, or "coining," as he terms it, of citizens by that same government by means of the Fifteenth Amendment to the Constitution.

The concepts that Michael O'Malley explores—so-called natural laws, appearance and inherent value, counterfeiting, banking, mobility and fixity, hard and soft money, freedom, gold, and whiteness—are central to nineteenth-century American culture, and he traces them through a variety of primary sources drawn

¹ Thanks to Alan Trachtenberg and Mary Kelley for thoughtful readings in pressing circumstances.

O'Malley collapses the controversies over fiat currency versus specie of the 1870s and gold versus silver in the 1890s, neglecting to note that David Wells's *Robinson Crusoe's Money*, first published in 1876 against greenbacks, was republished in 1896 against silver, and that silver and gold were both specie, although only gold wore the mantle of "civilization" in the 1890s. See Nell Irvin Painter, *Standing at Armageddon: The United States, 1877–1919* (New York, 1987), 83–90.

from several genres. Original and imaginative, this essay is an example of the nourishment of historiography by poststructuralism, and it will entertain and enlighten students of American culture. Although I have some serious reservations about his conclusions, my comments are mainly intended to advance his pursuit.

O'Malley's discussion of wildcat banking before the Civil War, notably his etymology of the word "carpetbagger," prefaces postwar dialogue in a new way. He sees Reconstruction as a moment of economic and social uncertainty, North and South, when the nation reunited politically and did its business with a monetary system invented during the war with the main aim of raising sufficient revenue to finance hostilities. The apparently gerry-built character of this system of greenbacks and national banks seemed, O'Malley says, to match the nouveau character of postwar politics. From the vantage point of the late 1860s and early 1870s, the Civil War appeared as a watershed that demarcated sharp changes in the political economy. Afterwards, it seemed, new men of little wealth and standing were taking over politics, manipulating patronage as a means of gaining wealth, and dismaying the aristocrats who before the war demanded deference and claimed that they had ruled disinterestedly. In the southern states, I would add along these lines, planters formed "taxpayers' conventions" consisting of wealthy men such as South Carolina's Wade Hampton III, who detested the idea that men lacking enough property to pay taxes should frame legislation for those who did. Who were the new men? Carpetbaggers and African Americans in the South, greenbackers and (unstated here but hinted at) Irishmen in the North and West, a few of whom called themselves workingmen and formed labor unions of a national scope.

This was indeed an altered political economy, but Viviana Zelizer, a sociologist of money, notes that its currency was more standardized than that of the antebellum era, when 5,000 banks were issuing notes of wildly varying soundness. During the war, federal policy began to tax state-chartered banks out of existence and substitute national bank notes and greenbacks for the plethora of currencies that had previously circulated. In terms of currency, at least, the postwar economy was simpler than its pre-war counterpart.² Compared with the early nineteenth century, the second half of the century saw less monetary anarchy, not a new "dizzying market freedom" in currency. This demurral, however, does not detract from the interest of O'Malley's archaeology of a symbol of Reconstruction.

Knowledge of the older association of "carpetbagger" with wildcat banking amplifies the resonance of this figure in Reconstruction, in which he became someone from afar who stirred up political trouble in the southern racial order. O'Malley is sensitive to the nuances of uncertainty pertaining to sound and unsound bank notes; the monetary side of the shared language of money and race is where his critical strength lies. But what of the other side? Given that discursive relationships surrounding bank notes and carpetbagging permit a novel assessment of patterns of postwar politics, might not this strategy work with regard to race? O'Malley might better have balanced the structure of his argument with

² Viviana A. Zelizer, "Making Multiple Monies," in *Explorations in Economic Sociology*, Richard Swedberg, ed. (New York, 1993), 197, 208.

more on the racial side of his language-of-money to language-of-race analogy, for the meaning of race before the Civil War also refracts the line of debate after the war.

THE RACIAL SIDE OF THE EQUATION of race and money is less figurative than the monetary, for the subject of race in the United States before the Civil War requires no metaphor to touch the language of money. Before 1865, the vast majority of African Americans were, literally, property, and they served simultaneously as an embodied currency and a labor force. As workers and as the basis of the economy in which they toiled, slaves circulated like legal tender. Fetishized as commodities, they embodied their owners' social prestige. Enslaved black people were not simply likened to money, they were a kind of money. As one of my students is finding, slaves not only could be bought (sometimes by the pound), sold, and rented in the market, their sales were also regulated by law and subject to warranties, trials, and return. Even more to the point semiotically, slaves were collateral for commercial, speculative, and personal loans.³ Enslaved persons, along with real estate, were property subject to exchange, and, as such, they undergirded the economies of eighteenth-century New York City as well as that of nineteenth-century southern states.

A glimpse of this equation appears in Michael O'Malley's essay. He quotes a *Petroleum V. Nasby* satire of 1867 by David Locke that links material to metaphor. The joke captures the convergence of black people—here, purposefully, women—and exchange, although the point defies O'Malley's larger argument.⁴ Less subtle and more cynical than O'Malley's analysis indicates, the *Nasby* story is about the literal value of sex in slavery, of women as sexual property, reproduction that has cash value, and the enhancement of that value when the act that engenders it joins men who were white with women who were black—sex that was bound to be coerced. These themes are inexplicably missing from O'Malley's discussion.

O'Malley broaches the crucial matter of miscegenation mainly through direct quotes, but Locke gets right to the sexual core of the iniquity that was slavery. By neglecting to mention any other potential white father, Locke hints that the master impregnates his black slave (her consent is not at issue) and perhaps impregnates his half-white daughters as well. The master is the father and incestuous grandfather of his property, who are children who must be sold in order for him to realize their cash value.

Contrary to O'Malley's allusion to certainties "as fixed as granite," the presence

³ Walter Johnson, "Masters and Slaves in the Market: Slavery and the New Orleans Trade, 1804–1864" (Ph.D. dissertation, Princeton University, 1994). Also invoking the metaphor of currency and bodily circulation that O'Malley notes, Johnson calls the slave trade "the lifeblood of the slaveholding economy."

⁴ O'Malley quotes *Nasby's* asking this examination question at a hypothetical southern university: "A high toned, shivilrous Virginian, twenty years ago, hed a female slave which wuz ez black ez a crow, and worth only \$800. Her progeny wuz only half ez black ez a crow, and her female grandchildren wuz sufficiently bleached to sell in Noo Orleans for \$2500 per female offspring. Required. 1st.: The length of time necessary to pay off the Nashnel debt by this means. 2nd.: The length of time required to bleach the cuss of color out of the niggers of the United States."

or absence of inherent racial differences was subject to noisy controversy before the Civil War. The monstrosity of owning one's kin—of family for sale in the marketplace—transcended racial lines and became a staple of the literature of both black and white abolitionists. Ex-slave narratives return time and again to the ways that slavery distorted family relations, a charge that apologists for slavery before and after the Civil War met mostly with silence, although when forced to account for the bleaching of black people, they blamed black women. O'Malley quotes a North Carolinian who speaks of miscegenation as white supremacists were wont to do: obliquely, in a speech meant to oppose black civil rights.

In boldfaced fashion, Locke grasps the sexual, reproductive, and racial (in the sense of color) aspects of slave property. The darker woman who is worth less can produce a woman who is worth more. If the master contributes further to the next reproductive step, the female grandchildren of the black woman, and only the girls, are each worth three times more than she. Locke locates the market in New Orleans, the symbolic site of the "fancy trade" in women who were sold for sex. The increase in value that Locke satirizes is not a function of increased labor power. It comes through the dilution of blackness in the female sex, for the sketch makes no mention of the market value of light-skinned men, even though the historical record may well turn up a parallel trade in fancy boys. Locke is not linking race and money, he is speaking of the ways in which race *was* money.

Published after emancipation in 1867, this sketch indicates that the memory of African Americans as an embodiment of money did not evaporate with the Confederate defeat. The recollection of people as property adds another level of meaning to the shared language of money and race, for those whom O'Malley quotes in the late nineteenth century were grown-ups during the time when race was money.

O'Malley mentions the North Carolinian August Merrimon to demonstrate the confluence of discourse, but Merrimon escapes his grasp and runs off in a direction that is rhetorically closer to Locke than to that of O'Malley's other hard money advocates. As the metaphors travel between O'Malley, Merrimon, and Locke, they collapse. Merrimon speaks of corruption and degradation when O'Malley wants him to talk of dilution, even though dilution, in Locke's scheme, leads to greater, not diminished value. Merrimon is thinking along Freudian lines that take him from color to race mixing to corruption to blackness to dirt, and from there, still with Freud, to money.⁵ The transfer point for Merrimon, as for Locke, is gendered reproduction.

As was so often the case, Merrimon displays castration anxiety over the loss of the sex-gender privileges that, in the antebellum era, rich white men had monopolized. This anxiety was provoked by the prospect of black men's freedom and citizenship, their manhood, in short, which entailed white men's concomitant loss. Merrimon does not phrase his terror of filth and contamination abstractly. He speaks in the first person singular his fears that the federal government, by prohibiting racial discrimination, will make "my skin black" and "corrupt my blood."

⁵ See Mary Douglas, *Purity and Danger: An Analysis of Concepts of Pollution and Taboo* (New York, 1966).

Castration shadows another of O'Malley's speakers, the Indianan Hugh McCulloch, Secretary of the Treasury, who, like Merrimon, opposed the enfranchisement of southern freedmen. For McCulloch, as for many conservatives who could not conceive of equality, hierarchies could never be disrupted; they could only be overthrown. If whites were not on top, then blacks must be. Enfranchisement for McCulloch was not a matter of black men's participation in politics, rather it was a question of their "control." In the context of congressional debate over Reconstruction, McCulloch, like Merrimon, spoke as someone with political prestige to lose. They inspire the characteristic queries of ideological critique and poststructuralism: who is speaking? and to what end?

IN O'MALLEY'S CONSTRUCTION, AN ANTHROPOMORPHIC "liberal individualism" or "free market liberalism" speaks, as in this statement: "it [free market liberalism] preached freedom in self-making, it [free market liberalism] also gave racial difference a fixed and non-negotiable meaning." To judge from the cast of characters, "liberal individualism" or "free market liberalism" is a fairly heterogeneous group that includes Thomas Hobbes, John C. Calhoun, Hugh McCulloch, James Pike, E. L. Godkin, Jacob Cox, the New York *Atlas and Argus*, David A. Wells, and James A. Garfield, but they do have something in common. Who, here, is speaking and what do they want? In O'Malley's framework, these are, after all, situated speakers: they are interested spokesmen who speak in a context. The men who talk O'Malley's "free market liberalism" believed in essences and intrinsic value, just as twentieth-century conservatives tout the tyranny of the genes to strengthen the rule of biological destiny as a means of forestalling change. In the 1860s and 1870s, those were the changes that abolitionists, feminists, and organized labor were demanding.

It is no accident that the speaker whom O'Malley quotes from Charles Chesnut's *Marrow of Tradition*, who regrets the new order of mere service (not deference) for wages, had been a slaveholding planter before the war. O'Malley's speakers were the George Fitzhughs of their society, and they wanted more than anything to discredit the claims of Susan B. Anthony and Elizabeth Cady Stanton for women's rights, of Frederick Douglass and Charles Sumner for black civil rights, of Wendell Phillips and William Sylvius for workingmen's rights, and of Alexander Campbell and Andrew Cameron for debtors and greenbackers. All of these people might also stake claims as voices of "free market liberalism" just as well as the conservatives O'Malley quotes.

When the Jacob Coxes and Wade Hampton IIIs of the time demanded that the "wealth and intelligence of the South" regain control, they were not theorizing idly. Faced with state legislatures full of poor men, black and white, who were finding ways of spending taxpayers' money, the Coxes and Hamptons saw themselves as representatives of wealth and intelligence, and they wanted, again, for themselves, to run the show and cut their taxes. Their language of essential qualities overflowed the bounds of race and money, and they saw sex and gender roles for women as similarly fixed by natural law. The same James A. Garfield who

valued the intrinsic worth of hard currency also believed that speaking in public or otherwise entering public life “desecrated” Woman. Such essentialism is older than the nineteenth century, where O’Malley finds it.

THE PHILOSOPHICAL DISCUSSION OF ESSENCES, which began in the sixth century B.C. in Ionia, antedates Aristotle (who embedded species and “whiteness” into the discussion) and John Locke (who linked gold with intrinsic qualities). Even though Bertrand Russell and a passel of nineteenth and twentieth-century feminists have seen the pursuit of essence as “a hopelessly muddleheaded notion,” it nonetheless lies at the center of Western philosophy as the study of metaphysics, ontology, and phenomenology.⁶ This pursuit of the meaning of substance and value extends much wider than nineteenth-century discussions of money and race, although money and human qualities have always figured prominently.

By attending only to the nineteenth century, O’Malley sees race and money as parallel essentialisms and misconstrues the structure of this symbolic process. A less narrow conception of the discourses of race and money can lead to larger generalizations that relate to Western culture, in which what pertains to the economy seems to be the same as what is real. Yes, race talk is full of economic symbolism, which in the nineteenth century was the language of currency. But Westerners phrase much more than race in the imagery of money and production. Western discourse generally—of gender and sex, notably—speaks in economic metaphor. A literary critic and an anthropologist have each written at length on this point.

Critic Marc Shell recalls that in ancient Greek culture the word for “word” (*Sēmē*) is the same as the word for “coin” and that coinage and tyranny appeared simultaneously. In an analysis of literature from the medieval era to the modern era, Shell notes that Georg Simmel, writing on the history of money, saw a similarity between the language of money and the forms of philosophy, and Shell strengthens Simmel’s conclusion. Money, says Shell, not only supplies a “root metaphor” to much Western literature, it also “talks in and through discourse in general.”⁷ The anthropologist phrases this conclusion slightly differently but with similar import.

Marshall Sahlins ends his discussion of Marxist practical logic and the cultural construction of consciousness by concluding that Westerners, including Americans, see the economic as what is fundamentally real and use the economy to mediate nearly everything else, including race. The economy, he says, is the “main site of symbolic production” and supplies the “major idiom of other relations and

⁶ Aristotle’s *Metaphysics*, John Warrington, ed. and trans. (London, 1956), Book Z, “Substance,” 174; John Locke, *An Essay Concerning Human Understanding*, Alexander Campbell Fraser, ed. (Oxford, 1894), Book 3, chap. 4, 56, 71, 79–83, 92–97; Bertrand Russell, *A History of Western Philosophy* (New York, 1945), quoted in David H. DeGroot, *Philosophies of Essence: An Examination of the Category of Essence* (Groningen, 1970), xi.

⁷ Marc Shell, *Money, Language, and Thought: Literary and Philosophical Economies from the Medieval to the Modern Era* (Berkeley, Calif., 1982), 2, 180–81, 186. See also Shell, *The Economy of Literature* (Baltimore, Md., 1978), 1–10.

activities.”⁸ For these two scholars, in sum, the relationship between the languages of money and race is not of parallel systems of metaphor but of a dominant site of cultural figuration, the economy, and one of many culturally constructed categories, race, to which the economy lends its symbolism. In O'Malley's place, they might see a sort of economic base and a racial superstructure.

Shell and Sahlins explain the convergence of imagery of race and money, but this still leaves the search for fundamental essences (which are not the same thing as intrinsic value, even though O'Malley tends to conflate them). Was this quest for certainty the product of a “paradox,” in O'Malley's words, of “free market liberalism,” the proof that in a time of economic flux, a place of stillness was required? This assertion, too, raises certain doubts, and not merely of the relative levels of monetary flux before and after the Civil War. Bertrand Russell summed up the search for unchanging essences as a quest for safety, for a “refuge from danger,” and it is this function that leads me back to the question of who is speaking in O'Malley's essay, to gender—this time, manhood—and class.

If O'Malley's voices of “free market liberalism” opposed black men's transfiguration from slaves to citizens, what is to be made of the rhetoric of manhood that people such as Congressmen Robert B. Elliott and Richard H. Cain invoked in order to support black men's civil rights? Black men, they said, had earned the vote through their service in the Union Army; by this logic, freedmen had not been coined passively into citizens by the Union but had actively forged the manhood that validated their enfranchisement. These were not merely quarrels over counterfeit men and money but also disputes about masculinity and the polity. O'Malley's conservatives, resisting change, offered up reasons why change must be resisted through an appeal to intrinsic value.

Within the language of race lay class conflicts, for, in the late 1860s, not only had most black people until recently been part of the economy as property and were, as such, the subjects of commodity fetishism, but they also belonged to the hardest working class in the country. With emancipation, freedpeople acted like free workers: they identified themselves as “a class of hard laboring people” and formed a National Colored Labor Union in tandem with the National Labor Union inspired by William Sylvis. In the 1860s and early 1870s, many erstwhile abolitionists, such as Frederick Douglass, Sojourner Truth, and Wendell Phillips, transferred their commitment to slaves to the welfare of workers. Phillips, in particular, became one of labor's truest defenders, supporting greenbacks as he supported labor unions, because he thought they were in the best interests of working people. Phillips did not speak the language of money, as O'Malley defines it, for Phillips, like others who tried to construe the world from a working-class angle, did not make a fetish of intrinsic value or obsess about the insubstantiality of paper money or black men as voters.

⁸ The quotes are from Marshall Sahlins, *Culture and Practical Reason* (Chicago, 1976), 211, see also 207, 210.

WHAT ALL THIS COMES DOWN TO is a convergence of the languages of race and money beyond what O'Malley can see out of the eyes of his conservatives: first, slavery embedded in the nineteenth-century American concepts of race a monetary connotation established during the many generations in which black people were property; second, the working-class status of African Americans as free people meant that Americans who advocated the interests of black people were also likely to support working people generally; and, third, Western culture as a whole privileges economic categories, which serve as a fertile source of metaphor. The nexus of race and money reveals a historical dialectic as well as a shared discourse. None of these conclusions is esoteric, and their very ordinariness raises the question of why they were invisible to so perceptive a historian as Michael O'Malley. Like many other thoughtful American academics, he is hobbled by the very theorists who were to set his thought free: European poststructuralists, whom I will let Michel Foucault embody for my purposes here.

One caveat: even though I have raised materialist concerns, this is not an argument against theory. Not in the least, but it is a warning of the limits of theory for Americanists. I am not referring to the more usual and well-grounded apprehension about the ways in which power dynamics can slip out of analyses that concentrate on discourse and the culturally determined shape of thought (although this does sometimes worry me).⁹ My reservations relate to the blind spots in the vision of prominent poststructuralists such as Michel Foucault with regard to race and slavery, matters on which they are almost uniformly silent.¹⁰ (They also tend to let class slide into the less spiked concept of power.) The point is not to castigate Europeans writing in the 1950s, 1960s, and 1970s for racism, for they help us illuminate crucial themes such as relations of power and knowledge (for example, Foucault's *Discipline and Punish: The Birth of the Prison* and *The History of Sexuality*, Volume 1). For all the usefulness of poststructuralists, however, they cannot fully elucidate American culture, which was conceived in slavery and fathered by slaveholders. I sometimes suspect that the silence of European poststructuralists is part of what draws Americans to their thought, for Europeans' networks of theory, by denying conceptual space to race and slavery, wordlessly abolish the need to theorize one of Americans' major historical embarrassments.¹¹ The postcolonial critic Homi Bhabha has similar misgivings about what poststructuralists have to say and not to say about their own national histories.

⁹ On power dynamics and theory, see, for example, Mary E. Hawkesworth, "Knowers, Knowing, Known: Feminist Theory and Claims of Truth," *Signs*, 14 (Spring 1989), reprinted in Micheline R. Malson, et al., eds., *Feminist Theory in Practice and Process* (Chicago, 1989), 327–51; and Susan Stanford Friedman, "Post/Poststructuralist Feminist Criticism: The Politics of Recuperation and Negotiation," *New Literary History*, 22 (Spring 1991): 465–90.

¹⁰ When poststructuralists such as Jacques Lacan occasionally touch on racial issues in passing—and it is seldom more than in passing, for race is not central to their thought—their speech is narrowed by the insensitivity of the 1950s and 1960s, when it was uttered. Lacan, for instance, mentions the "white-nigger notion of the total personality" and the "negress adorned for the wedding" virtually as asides. When he writes of slavery, his authority is Hegel rather than people who had actually been enslaved. See Lacan, *Ecrits: A Selection*, Alan Sheridan, trans. (New York, 1977), 133, 153, and 26, 42, 68, 80–81, 99, 142.

¹¹ See my "French Theories in American Settings: Some Thoughts on Transferability," *Journal of Women's History*, 1 (Spring 1989): 92–95.

Bhabha refers to Foucault's book *The Order of Things*, the very text that inspired Michael O'Malley, and is struck by Foucault's "massive forgetting" of colonialism in the evolution of the nineteenth-century Western episteme. Foucault has a lot of company when he obscures the context in which Westerners elaborated their grand narratives, which were, simultaneously, parables of mastery that appeared when and because subjected Others were fully in view as colonized subjects. Europeans, no less than Americans, are caught looking the other way when issues like race and empire beg to be integrated into theories of national experience and identity.¹²

Oddly enough, Foucault himself offers a way out. In a chapter in *The Order of Things* titled "Man and His Doubles," which might be construed in part as "European Man and His Doubles," he realizes that there exists a category of the "unthought." Foucault's unthought includes the unconscious, but that is not all there is to it. The unthought includes "dim mechanisms" and "faceless determinations," "an element of darkness," "an apparently inert density," "an obscure space." This Other is a brother and a twin that has accompanied "man" since the nineteenth century.¹³

Slaves are the unthought in the pages of Michael O'Malley's essay, for their literal economic-ness disappears from his analysis. In *Reconstruction*, he says, the meaning of racial difference was "renegotiated," as indeed it was, as the conservatives who had lost so much capital in the conversion of black people from property to humanity realized full well. But he does not think of the concrete meaning of people as property, of one's children being for sale, when he cites the relevant renegotiation. The unthought are unseen and unheard, their places usurped by those demanding certainty in a time of change. Unthought, too, is the notion that this formula issues from interested voices that insist on a place of safety, an assurance that things will change thus far and no farther. This "search for stability" is a quest that conservatives, not insurgents, undertake. Contrary to O'Malley's assertion, no iron law exists to regulate change; there is no objective decree that the freer market society becomes, the more it demands fixity of any sort, much less a fixity that is racial.

With regard to race and money, as with regard to every other thing, historians must not fail to ask who is speaking and what they want. Conflicting impulses inhabit every ideology, and, as O'Malley says, attention to language can reveal eloquent relationships, ruptures, and silences. European theorists, although they have their own myopias, can guide Americanists through transactions of language and culture. But until European poststructuralists face up to the theoretical implications of colonialism, Americanists dealing in the economy of race and slavery must learn to reckon on their own.

¹² Homi K. Bhabha, "Postcolonial Criticism," in *Redrawing the Boundaries: The Transformation of English and American Literary Studies*, Stephen Greenblatt and Giles Gunn, eds. (New York, 1992), 460–61.

¹³ Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (1966; New York, 1973), 326–27.

AHR Forum
Response to Nell Irvin Painter

MICHAEL O'MALLEY

NELL IRVIN PAINTER'S INVESTMENT OF TIME AND INTEREST redeems my speculations and places me in her debt. "Specie and Species" grew from a study of counterfeiting and racial passing, and, since the parallel discussion of money and citizenship that characterized Reconstruction offered an especially clear example of the shared language of money and race, most of my essay focuses on the 1870s. We agree that slavery, the commodification of racial difference, must be central to any future ventures in this line.

Slaves, Painter points out, "served simultaneously as an embodied currency and a labor force. As workers and as the basis of the economy in which they toiled, slaves circulated like legal tender. Fetishized as commodities, they embodied their owners' social prestige." The point is well taken; slaves occupied a unique and crucial role in antebellum political economy, and American historians cannot be reminded of this often enough. Acknowledging slavery's centrality, I would argue that slaves circulated not like legal tender but like specie. "Legal tender," deriving its value from law, pointed away from essentialism and toward the social and associative construction of meaning and value. But when they served as collateral for loans and embodiments of their owners' prestige, slaves served the same function as gold: their existence as valuable commodities anchored speculative enterprise, and their commodity value depended on fantasies of racial essentialism. On page 388, I suggest that the apparently chaotic antebellum financial system might only have been feasible in a society containing racial slavery. Would Painter agree that this might be true in the South—that racial slavery anchored speculative enterprise and that slaves helped stabilize a volatile, highly mobile, and economically expansionist society both socially and economically? This suggestion locates slavery at the core of American political economy and reconnects it to debates about money and banking.

In his famous message vetoing the Second United States Bank's recharter, Andrew Jackson linked a specie economy to a natural law meritocracy. "Distinctions in society will always exist under every just government," he insisted, because "equality of talents, of education, or of wealth can not be produced by human institutions." Some citizens enjoy "natural and just advantages," which a good government should do no more than help reveal. The bank and its paper money,

Thanks to Andrew Bush for his help with this reply.

he argued, represented an attempt to introduce "artificial distinctions, to grant titles, gratuities and exclusive privileges to make the rich richer and the potent more powerful." The veto speech describes the ideology of racial slavery as well as it does Jackson's idealized path from the backwoods to the White House—freedom and economic mobility for those intrinsically entitled to it by their "natural and just advantages," combined with an emphasis on legislation's inability artificially to alter fundamental nature. It allowed Jackson both to hold slaves and to maintain that, "in the full enjoyment of the gifts of heaven and the fruits of superior industry, every man is equally entitled to protection by law."¹ Slavery, a political economy that treated African Americans as specie, recapitulated the terms and assumptions of Jacksonian economic philosophy by linking biological essentialism to market freedom. Did northern workers derive the same "benefit" from race? Perhaps.² As Painter notes, the range of political opinion on both race and money was wider in 1832 than in 1873, and wider in both than space permitted me to document. But this premise, like my project as a whole, attempts to integrate racial and economic ideology, not to leave race or slavery unthought.

Slaves were, of course, primarily laborers, treated as commodities with a range of negotiable values and openly speculated in by traders. Painter rightly calls attention to the missing analysis of gender and the slave trade in my account of both Locke's joke and Merrimon's fears. True, Locke makes rape or concubinage central to his satire, and it is equally true that this sexual iniquity produced both greater and greater value in the "fancy markets" and greater and greater consternation in abolitionist literature. What seems most striking about the political economy of slavery is that literal commercial intercourse failed to erase the essential difference between white and black, at least in the minds of most whites. Achieving whiteness would render slaves unsalable and so a total economic loss; when "race" vanished, so did money. Although it does suggest a kind of "inflation" of black women's value, Locke's joke makes no sense unless the slaves remain black; unless "the cuss of color" could *never* be bleached out. It is this impossibly fetishized, essentialized racial identity that makes Merrimon scorn the idea of legislation diluting his blood—yet Painter is certainly right about the anxiety lurking beneath his brag.³

A racial convention that, as Barbara Fields puts it, "considers a white woman capable of giving birth to a black child but denies that a black woman can give birth to a white child" clearly connected racial identity to gender oppression.⁴ A

¹ "Message by President Andrew Jackson Vetoing the Bank Recharter, July 10 1832," reprinted in Herman E. Krooss, ed., *Documentary History of Banking and Currency in the United States*, 4 vols. (New York, 1983), 2: 36.

² The essays in David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (New York, 1991), give an especially useful account of racism's centrality to northern workers.

³ Merrimon's idealization of whiteness, oddly enough, depends on its having no commodity value—it cannot be traded; it holds its value irrespective of other economic fluctuations. Walter Benn Michaels' account of the gold fetish in *The Gold Standard and the Logic of Naturalism* (Berkeley, Calif., 1987), 148, suggests that, ironically, gold bugs wanted to imagine gold *never* functioning as money, removed altogether from the economy. Merrimon needs to imagine whiteness as outside trade, precisely because blackness is so closely identified with it.

⁴ Barbara J. Fields, "Ideology and Race in American History," in J. Morgan Kousser and James M.

political economy that has always privileged whiteness forced some Americans of mixed background to "pass" for whites, to make counterfeits of themselves. Again, the semantic linkage cannot be merely coincidental. "I had made up my mind that since I was not going to be a Negro," the narrator of James Weldon Johnson's *Autobiography of an Ex-Colored Man* declares, "I would avail myself of every possible opportunity to make a white man's success; and that, if it can be summed up in any one word, means 'money.'" In this instance, too, as Painter insists, race is money; whiteness is a coin Johnson's narrator can use to anchor his real estate speculations.⁵ While I heartily agree with Painter that we need more work on slave markets and the slave trade, America's peculiar insistence on racial essentialism also remains largely unexplained. I hope my examination of money and race offers some direction toward an explanation.

But that explanation does not depend on an "iron law regulating change" or an "objective decree" that the freer market society becomes, the more it demands fixity. I describe a paradoxical relation or tension between two oppositions that appear to reinforce each other—neither iron nor objective but historically contingent and fluctuating. Painter cites Wendell Phillips and a number of other "free market liberals" who speak against essentialism, worthy additions to those I mention myself. I cannot see how this refutes my repeated insistence that a desire for freedom in self-making coexists with a desire for fixity. P. T. Barnum's career alone should establish that Americans loved imposture, illusion, and the possibility of self-making through creative exaggeration every bit as much as they desired fixed identities. But Barnum's very slipperiness points to the idea of stability. His fakes require a "real," and his "real" leads us to imagine a fake. I never meant to imply that the logic of the free market made non-essentialist positions impossible; in fact, I argue that it produced them. Recovering that process through a careful analysis of the relation of ideology to interest, as Painter suggests, would be of immense value. There is much worth recapturing from the greenback or "associationist" tradition Phillips spoke from. But the recurring problem I hope to address is why freedom in self-making should be denied to people of color or to women on essentialist grounds—why race still tends to equal money and why African Americans still suffer the role of "doubles" for the figure of (white) "Western man." Would an emphasis on why men such as Phillips chose to argue against essentialism—an emphasis on "who spoke, and to what end"—explain the triumph of this essentialism? In the historical movement between idealized freedom and essentialized identity, neither an iron law nor a politically viable dialectical synthesis appears to emerge.

Elliot and Cain were trying to find one, one that left them open to neither racist essentialism nor fluctuations in the legislative climate: as Painter points out, their claims depended on a "rhetoric of manhood" that again related questions of racial and individual identity to gender politics. I hope Painter is right that a closer examination of the voices of slaves will point us toward an alternative to my dismal

McPherson, eds., *Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward* (New York, 1982), 149.

⁵ James Weldon Johnson, *Autobiography of an Ex-Colored Man* (1912; rpt. edn., New York, 1990), 141, 143.

science. I think immediately of Sojourner Truth's bared arm and her famous question—"a'n't I a woman? I have ploughed, and planted, and gathered into barns, and no man could head me! And a'n't I a woman?" Linking labor in the fields with labor in childbirth, she attacked essentialist notions of gender while demanding full, earned equality.⁶ I thank Nell Irvin Painter again for her generosity and her critical eye for timely and wise investment.

⁶ Sojourner Truth, quoted in Linda K. Kerber and Jane Sherron DeHart, eds., *Women's America: Refocusing the Past* (New York, 1991), 230.

AHR Forum
The Problem of American Conservatism

ALAN BRINKLEY

IT WILL NOT, I SUSPECT, BE A VERY CONTROVERSIAL CLAIM to say that twentieth-century American conservatism has been something of an orphan in historical scholarship. Historians have written books and articles about modern conservatism, of course, some of them quite good. In recent years, moreover, both the quantity and the quality of scholarship on the subject has markedly increased. Even so, it would be hard to argue that the American Right has received anything like the amount of attention from historians that its role in twentieth-century politics and culture suggests it should.¹ Given the history of the last twenty years, that is coming to seem an ever more curious omission. This essay is an effort to understand why that omission has occurred.

These observations are not the product of any personal scholarly research on conservatism (or of any personal engagement with or sympathy for conservative politics). On the contrary, my own recent work has focused on the history of American liberalism at mid-century. But this is not so abrupt a departure from such concerns as it might sound. I came to my study of American liberalism out of, among other things, a skepticism about some of the scholarly assumptions that have governed the study of American political culture in this century. Most historians have told the story of twentieth-century American political and cultural development by emphasizing the triumph of the progressive-liberal state and of the modern, cosmopolitan sensibility that has accompanied and to a large degree supported it. They have argued about the timing of this triumph and about

I presented an early version of this essay at a 1989 symposium on the twentieth-century Right at the University of Maryland, College Park, and I thank the members of the history department there both for the occasion and for their helpful responses. Similarly, I thank the history graduate student association at Yale, the department of history at Princeton, and the Columbia University Seminar on Twentieth-Century Politics and Society for opportunities to present later versions of the paper and for the many important suggestions and criticisms their members offered. John Higham, Michael Kazin, Gary Kulik, Marvin Gettleman, and Kathleen Blee generously commented on various versions of the manuscript; and Charles B. Forcey provided both research assistance and substantive suggestions. I am grateful to them all.

¹ In a recently published study of scholarship about the Right since the mid-1950s, William B. Hixson, Jr., claims to "have covered all the relevant scholarly material published by sociologists, psychologists, political scientists, and historians." Social scientists, he notes, have produced an enormous quantity of scholarship, but "historians as a group play a minor role in this study." Hixson, *Search for the American Right Wing: An Analysis of the Social Science Record, 1955-1987* (Princeton, N.J., 1992), xvii-xix.

whether it has been a good or bad thing. But, until recently at least, they have seldom doubted that it occurred.

Like some other recent scholars, however, I have been struck increasingly by other, quite different features of modern America: by the chronic weakness of the progressive state,² by the enormous difficulty liberals have had in securing and retaining popular loyalties, and by the persistent strength of other forces (many of which, for lack of a better word, we generally call conservative) in a long and still-unresolved battle over the nature of American politics and American culture.³ This is an important and, at least until recently, largely neglected part of the story of twentieth-century America. And so the "problem of American conservatism," as I define it here, is not a problem facing conservatives themselves and not a problem conservatives may have created for others. It is a problem of American historical scholarship, the problem of finding a suitable place for the Right—for its intellectual traditions and its social and political movements—within our historiographical concerns.

CONSERVATISM HAS NOT ALWAYS BEEN THE ORPHAN within American historical scholarship that it is today. The progressive historians who dominated the writing of American history through much of the first half of this century placed conservatives at the center of their interpretive scheme, a scheme that portrayed American history as a long and often intense struggle between popular democratic elements and entrenched anti-democratic interests. But theirs was a constricted view of conservatism, focused almost exclusively on economic elites and their efforts to preserve wealth and privilege. It is not surprising that later

² Skepticism about the progressive assumptions of much twentieth-century political history can be found in Barry D. Karl, *The Uneasy State: The United States from 1915 to 1945* (Chicago, 1983). Some more specialized studies that raise challenges to the assumption that the United States has been moving steadily toward greater political unity include Ellis W. Hawley, *The New Deal and the Problem of Monopoly* (Princeton, N.J., 1966), which chronicles the many frustrations New Dealers encountered in attempting to impose various forms of order on the industrial economy; Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities, 1877–1920* (Cambridge, 1982), which describes the halting, piecemeal process by which Americans "patched" together a modern state; James T. Patterson, *Congressional Conservatism and the New Deal* (Lexington, Ky., 1967), and Patterson, *The New Deal and the States: Federalism in Transition* (Princeton, 1969), which describe the obstacles to centralization the New Deal encountered and never entirely overcame; Alan Brinkley, "The New Deal and the Idea of the State," in Steve Fraser and Gary Gerstle, eds., *The Rise and Fall of the New Deal Order, 1930–1980* (Princeton, 1989), 85–121, which describes the way in which liberals modified or abandoned many of their most ambitious plans for consolidating and rationalizing the economy in response to substantial political, ideological, and economic obstacles.

³ Writers from both the Right and the Left have chronicled the survival of an anti-progressive, anti-statist tradition in twentieth-century America. Robert A. Nisbet, *The Quest for Community: A Study in the Ethics of Order and Freedom* (New York, 1953), is a conservative intellectual critique of the rise of the modern state and an account of the continuing struggle against its influence by individuals and non-governmental institutions and associations. The late Christopher Lasch, writing from the Left, has identified a tradition in American culture of important anti-progressive intellectuals (a tradition of which he was an outstanding contemporary example) in *The True and Only Heaven: Progress and Its Critics* (New York, 1991) and other works. The short-lived *democracy: A Journal of Political Renewal and Radical Change* (edited by Sheldon Wolin and with which Lasch was connected for a time) provided a running critique of progressive centralization and expressed the belief (and hope) that it was not, in fact, securely or inevitably entrenched as the governing dynamic of American life.

generations of scholars have found the progressive framework inadequate.⁴

What succeeded the progressive model, however, was a series of interpretive schemes that did relatively little to enlarge our understanding of conservatism and at times further marginalized the Right. That was particularly true of the so-called consensus scholarship that briefly dominated American historiography after World War II.⁵ The consensus scholars did take note of one of the most serious shortcomings of the progressive view of conservatism. They recognized that the Right did not consist only of elites defending wealth and privilege, that there was a popular, grass-roots Right—most immediately visible to them in the alarming rise of “McCarthyism” in the early 1950s—that needed explanation. But little in their explanations of what such scholars at times called the “radical Right,” the “New Right,” or the “pseudo-conservative revolt” suggested that conservatives were people whose ideas or grievances should be taken seriously or that the Right deserved attention as a distinct element of the American political tradition. Instead, the consensus approach tended to produce a dismissive view of conservatism, a view suggested by the literary critic Lionel Trilling’s famous 1950 statement, in the introduction to *The Liberal Imagination*:

In the United States at this time liberalism is not only the dominant but even the sole intellectual tradition. For it is the plain fact that nowadays there are no conservative or reactionary ideas in general circulation. This does not mean, of course, that there is no impulse to conservatism or to reaction. Such impulses are certainly very strong, perhaps even stronger than most of us know. But the conservative impulse and the reactionary impulse do not, with some isolated and some ecclesiastical exceptions, express themselves in ideas but only in action or in irritable mental gestures which seem to resemble ideas.⁶

Fourteen years later, in the midst of a presidential campaign that seems in retrospect to have challenged such assumptions, Richard Hofstadter (Trilling’s colleague and friend) wrote of Barry Goldwater that he “represents a very special minority point of view which is not even preponderant in his own party.” “When, in all our history,” Hofstadter asked, “has anyone with ideas so bizarre, so archaic, so self-confounding, so remote from the basic American consensus, ever got so

⁴ Perhaps the most influential version of the “progressive” interpretation of American history is Charles A. Beard and Mary R. Beard, *The Rise of American Civilization*, 2 vols. (New York, 1927). Among the discussions (and critiques) of progressive scholarship and the assumptions behind it are Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968); and Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (New York, 1988), esp. 92–108, 206–78.

⁵ Richard Hofstadter, *The American Political Tradition and the Men Who Made It* (New York, 1948), was one of the first expressions of what later came to be known as “consensus” assumptions. Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955), is perhaps the purest expression of those ideas, including a brusque dismissal of the influence of the Right: “The ironic flaw in American liberalism lies in the fact that we have never had a real conservative tradition” (p. 57). John Higham, “The Cult of the ‘American Consensus’: Homogenizing Our History,” *Commentary*, 27 (1959): 94–95, is the critical assessment that gave the “consensus school” its name. Novick, *That Noble Dream*, 320–60, examines and criticizes consensus scholarship.

⁶ Lionel Trilling, *The Liberal Imagination: Essays on Literature and Society* (New York, 1950), ix. Trilling was, however, more willing than many of his liberal contemporaries to concede that conservatism retained considerable strength in American culture, even as he dismissed it as a serious intellectual movement.

far?"⁷ The result of such assumptions was the tendency of consensus scholars to explain much American conservatism as if it were a kind of pathology—a "paranoid style," "symbolic politics," a product of "status anxiety"—an irrational or semi-rational aberration from a firmly established mainstream.⁸

But it was not just "consensus" scholars who had trouble taking conservatism seriously. New Left scholarship, which attacked the consensus with great effectiveness for ignoring or marginalizing the Left, had relatively little to say about the Right. That was in part because of the way much of the New Left celebrated, even romanticized, "the people." Having repudiated the liberal suspicion of "mass politics" and embraced instead the concept of "participatory democracy," scholars of the Left had difficulty conceding that mass movements could be anything but democratic and progressive; they found it difficult to acknowledge that they could emerge from the Right.⁹ But New Left scholars also neglected conservatism because, no less than the consensus historians they were challenging, they were in large measure preoccupied with the Cold War and the liberalism they believed supported it. One of the central assumptions of New Left political history, an assumption associated at first with William Appleman Williams and his students but one that ultimately spread far wider among radical historians, was that the ideology of capitalist hegemony in modern America has not been conventional conservatism but "corporate liberalism," which has shaped American foreign policy and domestic life alike.¹⁰ New Left political scholarship has, therefore,

⁷ Richard Hofstadter, "A Long View: Goldwater in History," *New York Review of Books* (October 8, 1964): 17–20. Peter Viereck, one of the contributors to *The New American Right* in 1955, had by 1963—when a second edition of this influential book was published—partially disavowed his own earlier, dismissive view of conservatism and by implication Hofstadter's as well. He had failed, he acknowledged, to recognize "something far more serious intellectually [than McCarthyism]—the non-McCarthyite, non-thought-controlling movement known as 'the new conservatism.'" Viereck, "The Philosophical 'New Conservatism,'" in Daniel Bell, ed., *The Radical Right: The New American Right Expanded and Updated* (Garden City, N.Y., 1963), 185.

⁸ The most celebrated expressions of the "consensus" approach to non-elite conservative dissent were the essays collected in Daniel Bell, ed., *The New American Right* (New York, 1955). Richard Hofstadter was the leading historical voice in this reassessment, especially in *The Paranoid Style in American Politics and Other Essays* (New York, 1965), which included his influential essay "The Pseudo-Conservative Revolt," first published in *The New American Right*. Clinton Rossiter, *Conservatism in America: The Thankless Persuasion*, 2d edn. (New York, 1962), is a partial exception to this trend. Rossiter never doubted the primacy of the liberal tradition in America, but he treated conservatism as a serious (if marginal) alternative intellectual stance. According to two recent, sympathetic historians of the Right, Rossiter "announced to the academic world that right of center was intellectually respectable." Charles W. Dunn and J. David Woodard, *American Conservatism from Burke to Bush: An Introduction* (New York, 1991), 4.

⁹ The ferocity with which the New Left attacked consensus scholarship for linking populism with such apparently anti-democratic movements as McCarthyism reflected, at least in part, this reluctance to accept that mass popular politics could embrace the Right. Scholarly work that attempted to rehabilitate the populists (and mass movements in general) includes Norman Pollack, *The Populist Response to Industrial America: Midwestern Populist Thought* (New York, 1962); Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976); Bruce Palmer, "Man Over Money": *The Southern Populist Critique of American Capitalism* (Chapel Hill, N.C., 1980). One of the first important challenges to the Hofstadter view of populism was C. Vann Woodward, "The Populist Heritage and the Intellectual," *American Scholar*, 28 (1959): 55–72, reprinted in Woodward, *The Burden of Southern History*, rev. edn. (Baton Rouge, La., 1968), 141–66. Michael Paul Rogin, *The Intellectuals and McCarthy: The Radical Specter* (Cambridge, Mass., 1967), argued, further, that support for McCarthy did not arise from "populist" impulses but from among traditionally right-wing, conservative groups.

¹⁰ William Appleman Williams suggested the outlines of the "corporate liberal" approach to

generally been more interested in discrediting liberalism—and, within the academic world, in wresting leadership and initiative from liberal scholars—than in confronting what it has generally considered a less formidable foe: the self-proclaimed Right.¹¹

Nor has the “organizational synthesis,” which has played a large role in recent years in shaping interpretations of twentieth-century America, found much room in its framework for the Right. The existence of a conservative tradition is not, perhaps, incompatible with the organizational view that the driving force in the modern world is the emergence of large-scale bureaucratic institutions. But neither does a conservative tradition play a very active role in that view. The organizational approach, therefore, tends to portray conservatism (when it considers it at all) in the same way it considers other forms of dissent: as the futile, and dwindling, resistance of provincial or marginal peoples to the inexorable forces of modernism.¹²

More recently, historians interested in the idea of “republicanism” have done much to revive scholarly interest in resistance to “progress” and the progressive state and to identify a powerful political tradition distinct from liberalism. Most scholarship on republicanism, however, identifies it as a set of ideas that *preceded* liberalism and ultimately fell victim to it—at the time of the American Revolution, according to some, and in the mid and late nineteenth century according to others. By the twentieth century, most such scholars imply, the nation’s ideological landscape was largely devoid of anti-progressive challenges to the liberal center.¹³

But to say that these and other interpretive models have left little room for the Right is not to answer the question of why historians have neglected conservatism. It is only to restate it. Why has American conservatism not claimed enough

twentieth-century history in his classic work *The Tragedy of American Diplomacy* (Cleveland, Ohio, 1959), but he gave fuller expression to the idea in *Americans in a Changing World: A History of the United States in the Twentieth Century* (New York, 1978). See also, among many other works, R. Jeffrey Lustig, *Corporate Liberalism: The Origins of Modern American Political Theory, 1890–1920* (Berkeley, Calif., 1982); and Martin Sklar, *The Corporate Reconstruction of American Capitalism, 1890–1916: The Market, the Law, and Politics* (New York, 1987).

¹¹ Robert Nisbet, recalling his own experiences as a conservative in the academic world of the 1960s, noted that “the Left never hassled me as they did the Kennedy liberals and also Old Socialists . . . In later years, other conservatives . . . told me their experience had been the same as mine; they were largely left alone, at least in comparison with those who weakly or despairingly kept trying to remind the Rudds and Savios of the nation that they were friends of the Revolution by other means.” *The Making of Modern Society* (New York, 1986), 17.

¹² One of the earliest and clearest statements of the assumptions of the “organizational synthesis” is Louis Galambos, “The Emerging Organizational Synthesis in Modern American History,” *Business History Review*, 44 (1970); a later evaluation is Galambos, “Technology, Political Economy, and Professionalization: Central Themes of the Organizational Synthesis,” *Business History Review*, 57 (1983): 472–93. A recent reconsideration is Brian Balogh, “Reorganizing the Organizational Synthesis,” *Studies in American Political Development*, 5 (1991): 119–72. A more skeptical view of the organizational synthesis can be found in Alan Brinkley, “Writing the History of Contemporary America: Dilemmas and Challenges,” *Daedalus*, 113 (1984): 121–42.

¹³ Daniel Rodgers, “Republicanism: The Career of a Concept,” *Journal of American History*, 79 (June 1992): 11–38, is a thoughtful, if skeptical, account of the influence of republicanism on modern scholarship. Gary Gerstle, *Working-Class Americanism: The Politics of Labor in a Textile City, 1914–1960* (New York, 1989), esp. 183–95, 331–36, argues that republican traditions remained hardy well into the twentieth century.

attention from scholars to cause them to revise or overthrow their conceptual models in order to make a place for it? How have scholars managed to content themselves with a set of paradigms in which conservatism plays so small a role? Answering those questions requires considering not just the nature of historiography but the nature of American conservatism itself.

AMERICAN CONSERVATISM IS NOT EASY TO CHARACTERIZE, even for those who view it sympathetically. Conservatism encompasses a broad range of ideas, impulses, and constituencies, and many conservatives feel no obligation to choose among the conflicting, even incompatible impulses, that fuel their politics. Individual conservatives find it possible, and at times perhaps even necessary, to embrace several clashing ideas at once. Conservatism is not, in short, an "ideology," with a secure and consistent internal structure. It is a cluster of related (and sometimes unrelated) ideas from which those who consider themselves conservatives draw different elements at different times. This ideological juggling makes the American Right particularly baffling to many of those historians who (as most do) stand outside it and try to make sense of it. Still, conservatism is no more inchoate than liberalism, progressivism, socialism, or any other broad political stance that describes a large and diverse group of people.¹⁴ And so its lack of ideological consistency and clarity is not a sufficient answer to the question of why it has received so much less attention than these other clusters of political ideas.

In the twentieth century, at least, American conservatism has also been relatively late in developing as a major intellectual or political force. (In this sense, there is at least some truth to Lionel Trilling's 1950 evaluation of the Right.) There have always been conservatives and reactionaries in modern America, but they have not always been very effective in making themselves heard or felt. George Nash, a sympathetic chronicler, has written that until at least 1945 "no articulate, coordinated, self-consciously conservative intellectual force existed in the United States. There were, at most, scattered voices of protest, profoundly pessimistic about the future of their country."¹⁵ Nor, prior to 1945, did American conservatives often constitute an effective political force, as the abysmal performance of such organizational efforts as the Liberty League in the 1930s suggests.¹⁶ Not until the postwar era did large numbers of conservatives manage to articulate a serious and important critique of liberal culture. And only in the 1970s did they begin to make that critique the basis of an effective political movement by creating (among other things) a network of publications, think tanks, and political action committees that have come to rival and often outper-

¹⁴ Daniel Rodgers has written provocatively about the diversity of meanings of conventional political labels in "In Search of Progressivism," *Reviews in American History*, 10 (December 1982): 113–32; and in *Contested Truths: Keywords in American Politics since Independence* (New York, 1987).

¹⁵ George H. Nash, *The Conservative Intellectual Movement in America: Since 1945* (New York, 1976), xiii.

¹⁶ George Wolfskill, *The Revolt of the Conservatives: A History of the American Liberty League, 1934–1940* (Boston, 1962); Robert F. Burk, *The Corporate State and the Broker State: The Du Ponts and American National Politics, 1925–1940* (Cambridge, Mass., 1990), esp. 143–253.

form their powerful liberal counterparts.¹⁷ Conservatism as an intellectually serious and politically effective movement is, in short, a relatively new phenomenon—born of the frustrations of political exile in the 1930s and 1940s, the passions of the anticommunist crusades of the late 1940s and early 1950s, and perhaps above all the political and cultural upheavals of the 1960s and 1970s. It has been slow to emerge in a visible and powerful-enough form to demand scholarly attention.

But this, too, seems an inadequate explanation for the absence of scholarly attention to the Right. If historians have done nothing else in the last twenty years, they have demonstrated their ability to retrieve the experiences of people and groups whose lives and ideas are not immediately visible in mainstream politics and culture. There have, for example, been long periods in the twentieth century when the Left has seemed dormant, in which its constituencies and goals were not immediately visible. And yet historians have very effectively portrayed the life and ideas of the Left during its years in the wilderness. The same case remains to be made for the Right.

A BETTER EXPLANATION for the inattention of historians may be that much American conservatism in the twentieth century has rested on a philosophical foundation not readily distinguishable from the liberal tradition, to which it is, in theory, opposed. Few historians any longer agree with Louis Hartz's claim that no important political theory has taken root in America unless it was grounded in a commitment to democratic capitalism and Lockean conceptions of freedom. But there was at least some truth in Hartz's claim, and the claim of scholars influenced by him, that "to be an American conservative it is necessary to reassert liberalism."¹⁸ Indeed, the defense of liberty, the preservation of individual freedom, has been as central to much of American conservatism in the twentieth century as it has been to American liberalism. Many conservatives would argue that in the twentieth century it has been much more central to their concerns than it has to the concerns of liberals.

That claim has some basis. Late nineteenth-century (or "classical") liberalism, epitomized in the ideas of the Liberal Republicans of the 1870s and 1880s,¹⁹ rested securely on the individualistic, anti-statist assumptions of John Stuart Mill and the Manchester liberals of England. What came to be known as "liberalism" in mid and late twentieth-century America has been to a significant extent a conscious repudiation of the anti-statist elements of that classical tradition. It has been, instead, an effort to build the case for a more active and powerful state (even if one in which ideas of individual rights played an important, often central role).

¹⁷ Sidney Blumenthal, *The Rise of the Counter-Establishment: From Conservative Ideology to Political Power* (New York, 1986).

¹⁸ Hartz, *Liberal Tradition in America*, 145–77; Peter Viereck said much the same thing in 1963 when he wrote that "our conservatism, in the absence of medieval feudal relics, must grudgingly admit it has little real tradition to conserve except that of liberalism." "Philosophical 'New Conservatism,'" 199. See also Stuart Gerry Brown, "Democracy, the New Conservatism, and the Liberal Tradition in America," *Ethics*, 66 (October 1955): 8.

¹⁹ John G. Sproat, *The Best Men: Liberal Reformers in the Gilded Age* (New York, 1968), 3–10, 143–68.

The anti-statist liberal tradition of nineteenth-century America has, therefore, increasingly become the property of those who in the twentieth century are generally known as conservatives (or, as some of them prefer, libertarians).

Nothing, in fact, so irritated many conservatives of the 1930s and 1940s as the New Deal's appropriation of the word "liberal." The real liberals, they insisted, were the enemies of New Deal statism, the defenders of individual rights against the "social engineering" and "paternalism" of the Left.²⁰ True liberalism, Herbert Hoover argued in 1938, rested on the "deep realization that economic freedom cannot be sacrificed if political freedom is to be preserved." The New Deal was not liberalism but a form of "national regimentation" reminiscent of fascism and communism. It was, Hoover argued, "a vast shift from the American concept of human rights which even the government may not infringe to those social philosophies where men are wholly subjective to the state. It is a vast casualty to Liberty if it shall be continued." Liberalism, which had emerged ascendant from World War I, "is today imperiled and endangered."²¹

Perhaps the single most influential contemporary statement of "conservative" opposition to the New Deal came from a man who always insisted he was a liberal, even as he became a hero to many right-wing intellectuals: Friedrich A. Hayek. Hayek was a distinguished Austrian economist who emigrated to England in 1931 and later moved to the United States, where he settled at the University of Chicago. With the specter of totalitarian oppression in Central Europe always in mind, Hayek devoted himself to refurbishing the tattered reputation of the classical, anti-statist liberalism of the nineteenth century. He became (along with others such as the economist Milton Friedman and the English philosopher Michael Oakeshott) an important voice on behalf of a form of libertarianism in modern society and a bitter critic of the "collectivism" he saw sweeping through Britain and America in the 1930s.²²

Out of these concerns emerged Hayek's celebrated 1944 book, *The Road to Serfdom*. It was not a work of scholarship, Hayek readily conceded. It was a "political book," a call to arms—a warning, directed at a general readership, of the dangers confronting the West. (He began writing it in London during the Nazi blitz, so it is perhaps not surprising that it had a superheated, polemical tone.) Somewhat implausibly, it became a major best seller, a *Reader's Digest* condensed book, and a Book-of-the-Month Club selection. To many postwar conservatives, Hayek's book served as a philosophical and even programmatic bible.²³

The Road to Serfdom was, at its heart, a strenuous polemic against the New Deal on what Hayek insisted were liberal grounds. The New Dealers, he claimed, offered fervent assurances that it was possible to increase the economic power of

²⁰ Michael W. Miles, *The Odyssey of the American Right* (New York, 1980), 18–20.

²¹ Herbert Hoover, *Addresses upon the American Road, 1933–1938* (New York, 1938), 138; Hoover, *The Challenge to Liberty* (New York, 1934), 103, 190.

²² C. Hartley Grattan, "Hayek's Hayride," *Harper's* (July 1945): 48–49; Hayek to Walter Lippmann, April 6, 1937, Lippmann Papers, Sterling Library, Yale University, Series III, Box 77.

²³ Friedrich A. Hayek, *The Road to Serfdom* (Chicago, 1944). For a discussion of the impact of the book on its time, see Theodore Rosenof, "Freedom, Planning, and Totalitarianism: The Reception of F. A. Hayek's *Road to Serfdom*," *Canadian Review of American Studies*, 5 (1974): 149–65. Nash, *Conservative Intellectual Movement in America*, 34–37, discusses the influence of Hayek's book on postwar American conservatives.

the state without infringing on personal liberty. But the totalitarian experiences of Germany and the Soviet Union illustrated the impossibility of maintaining that balance. "Economic control," he wrote, "is not merely control of a sector of human life which can be separated from the rest; it is the control of the means of all our ends." And, given that connection, the most dangerous form of economic control was statism, for "the separation of economic and political aims is an essential guaranty of individual freedom." Thus, he wrote, "It is necessary now to state the unpalatable truth that it is Germany whose fate we are in some danger of repeating." The United States, like Nazi Germany, had embarked on the "road to serfdom."²⁴

The centrality to modern conservatism of the essentially liberal concerns that Hayek raised—the fear of the state, the elevation of individual liberty above all other values, the insistence that personal freedom is inseparable from economic freedom—helps explain the dismissive view of conservative intellectual life among many liberal scholars. To them, this libertarian conservatism is simply a rigid and unreflective form of assumptions that liberals themselves share, not a fundamental or intellectually important challenge to the reigning political assumptions of American life.²⁵

That much of this individualistic conservatism has had a strong regional base has only added to the tendency of historians to dismiss it. Historians of the South, to be sure, have long acknowledged conservatism as a central element of their region's history. Indeed, in no other field of American scholarship have conservative ideas received such intensive and sophisticated analysis. Otherwise, relatively few scholars have until recently shown much interest in, or even recognition of, regionalism as a force in modern American history. According to many recent conceptual models, regionalism is a declining force, overwhelmed by economic centralization and mass culture. The history of modern conservatism—and, in particular, its close ties to the American West—suggests otherwise.²⁶

Conservatism has been an important presence in every area of the United States. But the dramatic rise of the Right in the last half-century may owe more to the West than to any other region. Of the most successful national conservative leaders of the postwar era, Barry Goldwater, George Wallace, Richard Nixon, and Ronald Reagan, all but Wallace were westerners. George Bush may or may not have been a genuine conservative, but it seems clear that he acquired his right-wing credentials (frail as they may have been) from his experience in Texas politics.²⁷ The most secure voting bloc (and the best source of money) for

²⁴ Hayek, *Road to Serfdom*, ix, 2, 92, 145–46.

²⁵ Examples of the liberal response to Hayek's book include Alvin Hansen, "The New Crusade against Planning," *New Republic* (January 1, 1945): 9–12, and Seymour E. Harris, "Breaking a Lance with Mr. Hayek," *New York Times Book Review* (December 9, 1945): 3, 14, 16.

²⁶ The emergence in the last decade or so of an energetic group of younger scholars engaged in a "new western history" has served as an important challenge to the tendency of many twentieth-century American historians to neglect regionalism as an important force in modern society. Two collections of essays that lay out some of the premises of the "new western history" are William Cronon, George Miles, and Jay Gitlin, eds., *Under an Open Sky: Rethinking America's Western Past* (New York, 1992); and Patricia Nelson Limerick, Clyde A. Milner II, and Charles E. Rankin, eds., *Trails: Toward a New Western History* (Lawrence, Kan., 1991). So far, however, relatively few of the new western historians have devoted much attention to the Right.

²⁷ Garry Wills, "The Hostage," *New York Review of Books* (August 13, 1992): 21–28.

conservative candidates and conservative causes has been the western states. Conservative intellectual life has found its most prominent homes at universities outside the East: the University of Chicago and, more recently, Stanford and other California universities.²⁸

One reason for this is the continuing distinctiveness of the West's social and economic circumstances and the particular appeal of conservatism's libertarian, anti-statist ethos to people dealing with those circumstances. Resentment of presumed domination by the East is one of the oldest themes in western American history. It has helped produce the populist revolt of the late nineteenth century and periodic movements of social and economic protest since. In the past half-century, moreover, many westerners have rechanneled the resentments that created populism—away from the great private economic institutions that were the traditional targets of western anger and toward the federal government, which many westerners believe has assumed the intrusive and oppressive role that banks and railroads once played as the great obstacle to western freedom. That should not, perhaps, be surprising. The federal government is the greatest landowner in the West. (It owns, for example, 44 percent of the state of Arizona, 90 percent of the state of Alaska). It controls an enormous proportion of the natural resources on which western economic growth largely depends. Many of its environmental regulations impinge on western preferences and western enterprises much more directly and severely than on their eastern counterparts. (The revolt against the 55 mile-per-hour speed limit in the 1980s was primarily a phenomenon of western libertarian conservatism, fueled by a sense of the disproportionate burdens the regulation inflicted on the region.)²⁹

That intrusive federal presence has been particularly difficult for many westerners to accept, because it has coincided with, and (according to many conservatives) obstructed, the West's rise to economic eminence. In reality, the West's rise to eminence is itself in large part a product of government largesse. Without the great federally funded infrastructure projects of the twentieth century—without the highways, airports, dams, water and irrigation projects, and other facilities the government has provided—the economic development of the Southwest in particular would have been impossible.³⁰ But few western conservatives have shown much inclination to confront such contradictions. They have focused not on those state initiatives benefiting them but on those they believe have served the declining East by curbing westerners' freedom to develop their own region. Kevin Phillips, an energetic chronicler (and, often, defender) of the modern Right, makes this point explicitly: that the West (or, to use the term he coined, the "Sun Belt") stands in dramatic contrast to the exhausted regions of the North and East, that it is the engine that can restore America's economic

²⁸ Richard White, *"It's Your Misfortune and None of My Own": A History of the American West* (Norman, Okla., 1991), 601–11, summarizes the history of the rise of the western Right.

²⁹ Michael McGerr, "Is There a Twentieth-Century West?" in Cronon, Miles, and Gitlin, *Under an Open Sky*, 248–50; Gerald D. Nash, "Bureaucracy and Reform in the West: Notes on the Influence of a Neglected Interest Group," *Western Historical Quarterly*, 2 (July 1971): 295–305.

³⁰ See, for example, Jordan A. Schwarz, *The New Dealers: Power Politics in the Age of Roosevelt* (New York, 1993); Leonard Arrington, *The Changing Economic Structure of the Mountain West, 1850–1950* (Logan, Utah, 1963); James L. Clayton, "The Impact of the Cold War on the Economy of California and Utah, 1946–1965," *Pacific Historical Review*, 36 (November 1967): 449–73.

greatness. He wrote in 1982: "I believe that the Sun Belt . . . is the key to making America work again both as a polity and as an economy," that "the frontier Frederick Jackson Turner believed closed is economically and spiritually open once more," that westerners' "churches, their businesses and their patriotism demonstrate the ongoing vitality of old American credos and self-reliant ways of doing things."³¹ The belief that unfettered economic freedom has been responsible for western economic growth—that, as Barry Goldwater has written, "individual initiative [has] made the desert bloom"—may be a myth.³² But, if so, it is a durable one, which fuels western conservatism and gives it a powerfully libertarian base.

LIBERTARIAN ASSUMPTIONS (REINFORCED AT TIMES BY REGIONALISM) have permeated modern American conservatism. But they have not constituted the whole of it. There are other powerful currents running through conservative thought, currents that have demonstrated growing power in the decades since World War II, that are not libertarian at all but intensely normative. Relatively few conservatives have been content to base their claims on purely libertarian grounds, and some, at least, have seen in America's "cult of liberty" a dangerous threat to civic virtue and social stability.³³ The forms these normative concerns have assumed, and the apparent contradiction between them and the libertarianism with which they coexist and in whose language they are often couched, have been especially difficult for historians to explain. This form of conservatism often seems intellectually inconsistent and hence resistant to analysis. Much more important, it has posed a direct challenge to some of modern liberalism's (and the modern Left's) most basic assumptions about the nature of modern American society.

The normative assumptions that long informed much European conservatism—the belief that a good society must find its grounding not simply in liberty but in respect for moral traditions, universal values, and inherited social hierarchies—did not find much favor in American thought through most of the nineteenth century or much of the twentieth.³⁴ To be sure, there were notable exceptions. Such ideas have always had a diffuse appeal to some privileged elites.³⁵ And they have had a special appeal to elites (and others) in the American South, which has throughout its history bred a number of defenses of hierarchical, organic notions of society, not only as rationalizations for white supremacy

³¹ Kevin P. Phillips, *Post-Conservative America: People, Politics, and Ideology in a Time of Crisis* (New York, 1982), 237–38; Phillips introduced the idea of the "Sun Belt" in *The Emerging Republican Majority* (New Rochelle, N.Y., 1969).

³² Barry M. Goldwater with Jack Casserly, *Goldwater* (New York, 1988), 35.

³³ See, for example, Robert Nisbet, "Conservatives and Libertarians: Uneasy Cousins," *Modern Age*, 24 (1980): 4–5; Nash, *Conservative Intellectual Movement*, 76–81.

³⁴ George Nash notes, for example, that "Burke was not highly esteemed in American academic circles in the 1930s" (*Conservative Intellectual Movement*, p. 69). Robert Nisbet observed similarly that Burke and Tocqueville were almost entirely absent from twentieth-century intellectual life, even among conservatives, until the 1940s (*Making of Modern Society*, p. 8).

³⁵ T. J. Jackson Lears, *No Place of Grace: Antimodernism and the Transformation of American Culture, 1880–1920* (New York, 1981), offers a provocative, and largely sympathetic, view of a form of normative cultural conservatism among elites in the late nineteenth and early twentieth centuries. See esp. chaps. 4, 5, and 7.

and economic oligarchy but also (as the Agrarians made clear in 1930 when they published *I'll Take My Stand*) as expressions of intellectual unhappiness with the progressive norms of the industrial world.³⁶ Historians, however, have generally explained the South's commitment to organicism and hierarchy as evidence of the region's distinctiveness, not as a sign of broader challenges to America's liberal core.

Until World War II, there was perhaps some justification for such assumptions. In the 1950s, however, a number of conservative intellectuals (many of them neither members of traditional elites nor southerners) launched a strenuous assault on relativistic and libertarian visions of society and built a case for the importance of inherited values and traditional norms that was not rooted in regional concerns.³⁷ One of the most influential was Russell Kirk, who claimed at times to have been influenced by the Agrarians and whose 1953 book *The Conservative Mind* ultimately became an important force in stimulating the growth of Burkean ideas in the American Right (and in encouraging the Right to appropriate Tocqueville as a source for its concerns). Kirk included among his six "canons of conservative thought" the "belief that a divine intent rules society as well as conscience" and that "political problems, at bottom, are religious and moral problems"; "affection for the proliferating variety and mystery of traditional life, as distinguished from the narrowing uniformity and equalitarianism and utilitarian aims of most radical systems"; the "conviction that civilized society requires orders and classes"; and the faith that "tradition and sound prejudice provide checks upon man's anarchic impulses." The "true conservative," he wrote,

may be a resolute and strong-minded clergyman, endeavouring, in his parish, to redeem men and women from their bondage to modern appetites, contending against all the power of the cheap press and the dreary cinema and the blatant radio, reminding them that they are part of a great eternal order, in which it is their lot to serve the ends of love and justice, venerating the mysterious social union of the dead, the living, and those yet to be born.³⁸

At about the same time, Leo Strauss and his disciples at the University of Chicago were offering a strong defense of Western and classical intellectual traditions as a source of eternal truths and timeless values. Modern social thought, Strauss argued, was not only incapable of improving on its classical forebears, it actually served to erode the moral and intellectual foundations of civilization. Liberal political theory, with its emphasis on individual liberty and subjective morality and its eager rejection of "natural right," leads, Strauss wrote, "to

³⁶ *I'll Take My Stand: The South and the Agrarian Tradition*, by Twelve Southerners (New York, 1930).

³⁷ Paul Gottfried and Thomas Fleming, *The Conservative Movement* (Boston, 1988), give special attention to the normative qualities of modern conservatism. "Conservatives, as much as Leftists," they write, "are united by a distinctive approach to reality—particularly nature. For the Left, the concept 'nature' suggests infinite plasticity; for the Right, by contrast, it is something fixed, and even normative" (ix–x).

³⁸ Russell Kirk, *The Conservative Mind, from Burke to Santayana* (1953; London, 1954), 17–18, 140–65, 436. On the positive reaction to Kirk among conservative intellectuals, see Nash, *Conservative Intellectual Movement*, 74. Kirk's book followed, and built on, other, less widely noted texts of the postwar period that offered similar defenses of a more normative conservatism. See, for example, Nisbet, *Quest for Community*; Richard M. Weaver, *Ideas Have Consequences* (Chicago, 1948); Peter Viereck, *Conservatism Revisited: The Revolt against Revolt, 1815–1949* (London, 1949).

nihilism—nay it is identical with nihilism.” For, he argued, “Once we realize that the principles of our actions have no other support than our blind choice, we really do not believe in them any more. We cannot wholeheartedly act upon them any more. We cannot live any more as responsible beings . . . The more we cultivate reason, the more we cultivate nihilism.”³⁹ The effort to root principles in particular historical circumstances, to deny the existence of “universal norms,” to resist “efforts to transcend the actual”—an effort that formed the basis of the modernist project—had, Strauss claimed, produced a society in which no principle or value could withstand attack. The attempt by historicists “to make man absolutely at home in this world ended in man’s becoming absolutely homeless.”⁴⁰

Neither Kirk nor Strauss enjoyed wide recognition or acclaim in the 1950s, when they were doing their most important work. Kirk remained for many years an isolated, largely unread cult figure (with a very small cult); and Strauss, in part because of his frequent obscurantism and his highly elitist views about the proper audience for philosophical ideas, developed no significant following beyond the fervent circle of admirers he collected (and retains) within academia.⁴¹

But other intellectual defenders of normative conservatism attracted considerable attention. Catholic conservative intellectuals (perhaps most prominently, William F. Buckley) have long attacked the relativism and excessive individualism of modern liberalism. At times, they have renounced industrialism altogether and have turned instead to an image of a preindustrial world in which the bonds of community were sustained by timeless values protected by the church.⁴² Some have drawn from the church’s invigorated twentieth-century interest in Thomas Aquinas and Aquinas’s ideas of an organic community, ideas the Catholic Left has used at times as well.⁴³ Major writers and artists of the first half of the twentieth century (among them T. S. Eliot and Willa Cather) rebelled against the relativism and the acquisitive, materialistic values of modern industrial society, a stance vigorously defended in the 1960s and beyond by, among others, Saul Bellow. “The tendency of unlimited industrialism,” Eliot wrote in 1939, “is to create bodies of men and women—of all classes—detached from tradition, alienated from religion, and susceptible to mass suggestion. And a mob will be no less of a mob if it is well fed, well clothed, well housed, and well disciplined.”⁴⁴ Some

³⁹ Leo Strauss, *Natural Right and History* (Chicago, 1953), 4–5.

⁴⁰ Strauss, *Natural Right and History*, 16–18.

⁴¹ Alan Udoff, ed., *Leo Strauss’s Thought: Toward a Critical Engagement* (Boulder, Colo., 1991), contains recent, appreciative critiques of Strauss’s work. But Shadia B. Drury, *The Political Ideas of Leo Strauss* (Houndsmill, Eng., 1988), and Stephen Holmes, “Truths for Philosophers Alone?” *Times Literary Supplement* (December 1–7, 1989): 1319–24, are less sympathetic.

⁴² William F. Buckley, Jr., *God and Man at Yale: The Superstitions of “Academic Freedom”* (Chicago, 1951), was perhaps the most visible example of this Catholic social conservatism in the early postwar period. See also Ross Hoffman, *The Organic State: An Historical View of Contemporary Politics* (New York, 1939). Patrick Allitt, *Catholic Intellectuals and Conservative Politics in America, 1950–1985* (Ithaca, N.Y., 1993), is an important recent study.

⁴³ George Q. Flynn, *American Catholics and the Roosevelt Presidency, 1932–1936* (Lexington, Ky., 1968), 22–35. The revived interest in Aquinas was sparked, or at least signaled, by the 1891 publication of Leo XIII’s encyclical *Rerum novarum* or *On the Condition of the Working Class* and reinforced by Pius XI’s 1931 encyclical *Quadragesimo anno* or *Forty Years After: On Reconstructing the Social Order*; see Leo XIII and Pius XI, *Two Basic Social Encyclicals* (New York, 1943).

⁴⁴ See, for example, Eliot’s 1939 lectures at Corpus Christi College, Cambridge, published as *The Idea of a Christian Society* (London, 1939); quotation is from p. 21. Saul Bellow’s attraction to a nor-

Jewish intellectuals have cited the religious and civic traditions of Judaism in an attack on what they consider the excessive emphasis on individual rights and liberties in modern American liberalism; a successful society, they believe, must rest on a set of moral standards shared, and if necessary enforced, by the community.⁴⁵

In the 1970s and 1980s, this normative intellectual tradition began to attract a large political and even popular following. The Straussians, for example, were at the center of the intellectual and academic debates of the 1980s, and (as the enormous success of Allan Bloom's book *The Closing of the American Mind* demonstrates) they won considerable sympathy for their argument that tradition can provide society with a much-needed moral and spiritual core.⁴⁶ The so-called neo-conservatives, most of them former socialists, began in the 1960s to embrace and promote a form of normative conservatism in their effort to discredit the New Left. They did not, on the whole, embrace Strauss or Kirk. But their denunciations of the radicalism and relativism of the 1960s, their calls for a relegitimation of traditional centers of authority, and their cries for a refurbishment of American nationalism and a recognition of the moral claims of American democracy came increasingly to resemble the appeals of other, more longstanding conservatives.⁴⁷ (Their appeals have found an echo as well among a group of intellectuals—sometimes described as “neo-liberals”—who on social and cultural issues at least have adopted normative stances in many ways similar to those of some self-proclaimed conservatives.⁴⁸)

native conservatism is evident in his admiring, if somewhat guarded, introduction to Allan Bloom, *The Closing of the American Mind* (New York, 1987), 11–18.

⁴⁵ Daniel Bell, for example, told an interviewer in 1978 of “a fear of mass action, a fear of passions let loose. A lot of this goes back to a particularly Jewish fear. In traditional Jewish life, going back particularly to the Assyrian and Babylonian episodes . . . there's a fear of what happens when man is let loose. When man doesn't have halacha, the law, he becomes chia, an animal.” Nathan Liebowitz, *Daniel Bell and the Agony of Modern Liberalism* (Westport, Conn., 1985), 70.

⁴⁶ Richard Rorty and Harvey C. Mansfield, Jr., debate the implications of Bloom's book in “Straussianism, Democracy, and Allan Bloom,” *New Republic* (April 4, 1988): 28–37.

⁴⁷ Irving Kristol, *On the Democratic Idea in America* (New York, 1972), 330–33; Phillips, *Post-Conservative America*, 5–49. Robert A. Nisbet, *Twilight of Authority* (New York, 1975), especially in chapter 5, suggests the confluence of neo-conservative thought with an older tradition of normative conservatism. Critical discussion of the neo-conservative movement includes Peter Steinfels, *The Neo-Conservatives: The Men Who Are Changing America's Politics* (New York, 1979); Nathan Glazer, Peter Steinfels, James Q. Wilson, and Norman Birnbaum, “Neoconservatism: Pro and Con,” *Partisan Review*, 47 (1980): 497–521; Amitai Etzioni, “The Neoconservatives,” *Partisan Review*, 44 (1977): 431–37; Seymour Martin Lipset, “Neoconservatism: Myth and Reality,” *Society*, 25 (July–August 1988): 29–37.

⁴⁸ See, for example, Michael J. Sandel, *Liberalism and the Limits of Justice* (Cambridge, 1982); Michael Walzer, *Spheres of Justice: A Defense of Pluralism and Equality* (New York, 1983). These new critiques of liberalism, many of them ostensibly from the Left, are often difficult to distinguish from neo-conservative and even older conservative critiques. Some, for example, have much in common with the work of the English political theorist Michael Oakeshott, a major spokesman for an older conservative tradition. Jeremy Waldron, for example, notes a similarity between Oakeshott's description of a modern, liberal rationalist (“Like a man whose only language is Esperanto, he has no means of knowing that the world did not begin in the twentieth century”) and Sandel's (“a person wholly without character, without moral depth, for to have character is to know that I move in a history I neither summon nor command”). Waldron, “Politics without Purpose?” *Times Literary Supplement* (July 6–12, 1990): 715–16.

IF THE ARGUMENTS OF THESE CONSERVATIVE INTELLECTUALS were the whole, or even the most important part, of the normative conservatism of recent years, historians would probably have relatively little difficulty explaining and categorizing their ideas. But there is another segment of the contemporary Right whose demands are considerably more radical and whose critique of the contemporary world derives not from elitist notions of tradition and morality but from what, for lack of a better term, might be called a deep-seated cultural and religious fundamentalism.⁴⁹ This is, in the end, what has constituted the greatest "problem" of American conservatism (for historians and for liberal culture in general): the challenge of understanding and explaining a phenomenon so profoundly at odds with what many Americans have come to believe are the uncontested assumptions of modern Western society.

The dramatic resurgence of fundamentalism as a social and political force took almost all liberals (and almost all historians) by surprise when it became visible in the 1970s. The goal of the fundamentalist Right was to challenge the secular, scientific values of modern culture, values most liberals have come to consider norms of modernity. Many liberals were, therefore, surprised and even baffled by the suddenly powerful assaults on such symbols of "progress" as the secularization of popular culture, the teaching of evolution, even the principle of the separation of church and state.⁵⁰ Fundamentalists revived ancient quarrels over the banning of books and movies. Some used religious arguments to frame positions on seemingly non-religious issues, claiming, for example, that the Bible mandated a massive expansion of the American defense budget (an argument that Ronald Reagan, on occasion, seemed to endorse). Others argued that biblical prophecies of the coming millennium should be a factor in the shaping of public policy (a view that Secretary of the Interior James Watt once cited in defense of his opposition to environmental regulations).⁵¹ Many mixed their religious fervor with an essentially secular fundamentalism, which rested on a normative view of "traditional" middle-class constructions of family, community, and morality.

Indeed, the most powerful single strain within fundamentalist conservatism through much of the 1970s and 1980s may have been its assault on the efforts of modern feminists to redefine gender roles. Battles over abortion, birth control, the Equal Rights Amendment, and other gender-based issues (and, more recently, battles over homosexuality) have mobilized the fundamentalist Right more successfully and energetically than any other issue.⁵² Anti-feminist women were

⁴⁹ I use the term "fundamentalism" not simply to describe people with fundamentalist religious beliefs (some of whom are involved in the so-called Christian Right but many of whom are not politically active) but a larger group, often described as the "New Right" or the "populist Right," who share a commitment to purging American culture and politics of what they consider its relativistic, anti-traditional character.

⁵⁰ "The conflict," writes the Christian conservative Carl Horn, "is between those who believe that law and public policy derive from religious belief and those who reject such an assumption." Gottfried and Fleming, *Conservative Movement*, 85.

⁵¹ Paul S. Boyer, *When Time Shall Be No More: Prophecy Belief in Modern American Culture* (Cambridge, Mass., 1992), 140–51, 304–24.

⁵² Jane Sherron De Hart has contrasted the normative quality of the anti-feminist position to the more relativistic stance of feminists. "Gender," she writes (characterizing the right-wing position) "was sacred. It was a given: a biologically, physically, spiritually defined thing; an unambiguous, clear, definite vision of humanity into two. Feminists, however, insisted that gender, like race, was a social

especially active in the revival of the fundamentalist Right. And they were instrumental in tying it to two important and related claims: that a family structure rooted in traditional notions of gender is the basis of a stable, moral society, and that a moral consensus in society is, in turn, essential to the stability of the family. "The family is the core institution that decisively determines the nature of society itself," one pro-family activist wrote in 1980. It is, she insisted, "the primary source of moral authority for the developing individual. The moral authority anchored in the family is by its very nature dependent on a consensus of core values within society." In that light, the social changes of the 1960s and 1970s appeared a menacing threat to what anti-feminists believe were once universally accepted norms. Prior to the 1960s, one activist argued in 1982,

America was accepted as being a Christian nation . . . This country was then very much family-oriented. Though divorce, living together outside of marriage, abortion, homosexuality were not uncommon then, they were at least seldom defended in theory. There were moral absolutes that were recognized; and agencies of public expression, including the media and school system, honored those values. Then, unfortunately, over the last twenty years this Judeo-Christian moral consensus has been threatened, challenged, and often times shattered.⁵³

This newly powerful challenge to secular culture has been all the more puzzling to many liberal and leftist scholars because its champions have often couched their essentially normative demands in libertarian language: denouncing a coercive state or an alien "cultural elite" for intruding on the lives of individuals and communities. But the liberal rhetoric should not obscure the larger agenda of most politically active fundamentalists (religious and secular alike), which is not just to protect their own allegiance to "traditional" moral standards but to impose them on society as a whole.

By the early 1980s, it was no longer possible to dismiss conservative fundamentalism in America as a declining rural peculiarity, consigned to oblivion a half-century ago by the Scopes trial and the inexorable forces of modernization. It was necessary to recognize it as a considerable and growing social and political force, which was finding expression at times at the heart of the American state. And while the highly publicized setbacks of some of the most prominent religious fundamentalists of the 1980s considerably weakened their political power, the fundamentalist Right remains a potent, even growing, political force in America (as the 1992 Republican convention suggested), just as it is an important and rapidly growing force in many other areas of the world.⁵⁴

construction; the meaning attached to sexual difference was actually made by humans, not God or Nature, and therefore could be changed." De Hart, "Gender on the Right: Meanings behind the Existential Scream," *Gender and History*, 3 (Autumn 1991): 256. See also Kristin Luker, *Abortion and the Politics of Motherhood* (Berkeley, Calif., 1984), 158–75; Zillah R. Eisenstein, "The Sexual Politics of the New Right," *Feminist Theory*, 7 (Spring 1982).

⁵³ Rebecca E. Klatch, *Women of the New Right* (Philadelphia, 1987), 23–25. The statements, quoted in Klatch, are by Onalee McGraw and Virginia Bessey.

⁵⁴ Martin E. Marty and R. Scott Appleby have edited a series of volumes, known collectively as the Fundamentalist Project, at the University of Chicago Divinity School in an effort to identify both the international character of fundamentalism and the wide-ranging utility of the concept. Three volumes were in print as of mid-1993, all published by University of Chicago Press: *Fundamentalisms*

In fact, as George Marsden and other historians of modern evangelicism have revealed, fundamentalism and pentecostalism—the two religious movements that have had the most impact on politics and culture in recent years—were never the dwindling rural phenomena that scholars had assumed in the 1950s and 1960s. The traumatic experience of the Scopes trial stilled the *public* voice of fundamentalism for a time; but throughout the 1930s, and even more aggressively in the 1940s and 1950s, fundamentalist and pentecostal denominations were growing more rapidly than any other religious orders.⁵⁵ What happened in the 1970s was not so much a sudden explosion in the number of evangelicals in America (although that happened, too) but a renewal of cultural and political activism within an already large and well-established religious community.

The resurgence of right-wing fundamentalism in the United States has unsettled many liberal and left-oriented scholars because it has seemed to contradict some of their most basic assumptions about modern society. A rational, economically developed society, progressive intellectuals have tended to believe, does not spurn modernity. It does not reject progress. Liberal scholars have tended to explain the phenomenon by stressing economic backwardness and a kind of cultural irrationality, by emphasizing the oddities of the fundamentalist mind and the idiosyncrasies (or pathologies) of provincial cultures. Leftist scholars have stressed class oppression and economic deprivation but have reached essentially the same conclusion: that fundamentalism was a product of social or economic isolation and powerlessness. The fundamentalist Right, scholars have been tempted to believe, is a group somehow left behind by the modern world—economically, culturally, psychologically—expressing frustration at their isolation and failure.⁵⁶

And yet the reality of modern American fundamentalism, as recent scholarship has begun to demonstrate, is not always, or even usually, congruent with these assumptions. Who, in fact, have been the men and women who have populated the fundamentalist and pentecostal churches, who have protested against “godlessness” and “immorality” in popular culture, who have adopted obscurantist positions on education and publishing, who have joined fundamentalist political crusades to make America a “Christian nation”? Many, perhaps most, of these people have not been poor, provincial folk or helpless victims of economic oppression. They have not been an isolated, rural fringe. They have not been

Observed (Chicago, 1991); *Fundamentalisms and Society: Reclaiming the Sciences, the Family, and Education* (Chicago, 1993); and *Fundamentalisms and the State: Remaking Politics, Economies, and Militance* (Chicago, 1993). See also Malise Ruthven, “The Fundamentalist Project,” *Times Literary Supplement* (April 30, 1993): 14.

⁵⁵ George M. Marsden, *Fundamentalism and American Culture: The Shaping of Twentieth-Century Evangelicalism, 1870–1925* (New York, 1980), 176–98.

⁵⁶ A classic example is Seymour Martin Lipset and Earl Raab, *The Politics of Unreason: Right Wing Extremism in America, 1790–1970* (New York, 1970), written before the resurgence of the Right in the 1980s and largely concerned with right-wing movements of the 1930s, 1940s, and 1950s. Daniel Bell’s book *The New American Right* was reissued in 1963 under the title *The Radical Right* (Garden City, N.Y., 1963), with new essays by Bell and others reinforcing the implication of the earlier volume that the normative Right was the product of social and psychological dislocation. Among other examples of such analyses, see Dean M. Kelley, *Why Conservative Churches Are Growing: A Study in Sociology of Religion* (New York, 1972). For an extensive discussion of this literature, see Hixson, *Search for the American Right Wing*, 9–26, 61–112.

rootless, anomic people searching for personal stability. To an increasing extent in the last fifty years, they have been people who have moved successfully into at least the lower ranks of the middle class, and sometimes much higher; people who have shared in the fruits of the consumer culture; people who have become part of the bureaucratized world of the organizational society. Many of them have been people with stable families and secure roots in their communities; people from urban areas, members of the new service economy, men and women whose new affluence has not weakened their fundamentalist beliefs. If anything, it may have strengthened them.⁵⁷

Recent scholarship on the Ku Klux Klan of the 1920s and 1930s makes clear that much the same can be said about its membership, which consisted heavily of urban, middle-class men and women.⁵⁸ Scholarship on right-wing political dissenters in the 1930s suggests that even some of the most extreme and, on the surface, bizarre political leaders of the Depression years attracted supporters who were in all visible respects stable, rational, "normal" people whose deep resentments against the modern world were not rooted in social or economic marginality. That such men and women so often combined a defense of their own moral values with a populist resentment of distant centers of corporate or state power further reinforces this emerging picture of people whose political views are in many ways as fluid, adaptable, pragmatic, and internally inconsistent as any other group and refutes simplistic images of them as obsessive zealots.⁵⁹

The nature of the modern fundamentalist Right suggests, in short, that it has been possible to be a stable, affluent, middle-class person, to have become part of the modern bureaucratic world and to have embraced the consumer culture, to have achieved and enjoyed worldly success, and to have clung nevertheless to a set of cultural and religious beliefs that are at odds with some of the basic assumptions of modernism.⁶⁰ And these possibilities serve as a challenge to the assump-

⁵⁷ Recent work on religious fundamentalism and pentecostalism that supports such a conclusion includes Marsden, *Fundamentalism and American Culture*; Gillian Peele, *Revival and Reaction: The Right in Contemporary America* (New York, 1984); David Edwin Harrell, Jr., *Oral Roberts: An American Life* (Bloomington, Ind., 1985); Harrell, *All Things Are Possible: The Healing and Charismatic Revivals in Modern America* (Bloomington, 1975). David H. Bennett, *The Party of Fear: From Nativist Movements to the New Right in American History* (Chapel Hill, N.C., 1988), chronicles the history of the many nativist movements (some of them with fundamentalist roots) that have surfaced repeatedly throughout American history.

⁵⁸ Two important new books that explore the social roots of the Klan and dispute earlier, "paranoid-style" characterizations are Leonard J. Moore, *Citizen Klansmen: The Ku Klux Klan in Indiana, 1921-1928* (Chapel Hill, N.C., 1991); and Kathleen M. Blee, *Women of the Klan: Racism and Gender in the 1920s* (Berkeley, Calif., 1991). Kenneth T. Jackson, *The Ku Klux Klan in the City, 1915-1930* (New York, 1967), was an early challenge to the portrait of the Klan as a rural, provincial peculiarity.

⁵⁹ Alan Brinkley, *Voices of Protest: Huey Long, Father Coughlin, and the Great Depression* (New York, 1982), considers two protest movements of the 1930s that have often been considered right-wing and even fascist and suggests that they were rooted in rational economic grievances and a broad populist sensibility. Leo P. Ribuffo, *The Old Christian Right: The Protestant Far Right from the Great Depression to the Cold War* (Philadelphia, 1983), considers several of the most extreme right-wing leaders of the interwar years and suggests that even their unattractive views were not radically at odds from those of mainstream Americans.

⁶⁰ Among important recent work that suggests the stability and rationality of the non-elite Right is Jerome L. Himmelstein, *To the Right: The Transformation of American Conservatism* (Berkeley, Calif., 1990); Klatch, *Women of the New Right*; Jonathan Rieder, *Canarsie: The Jews and Italians of Brooklyn against Liberalism* (Cambridge, Mass., 1985); Ronald P. Formisano, *Boston against Busing: Race, Class,*

tions of most historians that intense religious faith and fundamentalist morality should be understood as secondary or dependent characteristics, products of economic or social maladjustment, to be discarded as their adherents move into the cosmopolitan world. The character of the modern fundamentalist Right suggests that faith and normative morality may, instead, be primary characteristics, with autonomous power. That they survive and flourish in the midst of a culture many historians (and others) have assumed is incompatible with them suggests that it is possible to live in the modern world and enjoy its largesse without absorbing modernist values. They suggest that the integrated economy and powerful mass culture of modern America may not have the homogenizing power that many critics have assumed.

Robert Wiebe, an important figure in the growth of the "organizational synthesis," suggested in a small and largely unnoticed book published in 1975 that the nationalizing and consolidating forces he had described so effectively in *The Search for Order* may not be sufficient to explain the nature of modern society. The United States, he argued, is not a truly consolidated nation. It is, in the phrase he uses as the title of his book, a "segmented society." The American people, Wiebe argued, live in a nation of almost unparalleled diversity and complexity. They cope with that diversity less by rallying behind common assumptions and universal values than by "segmenting" their world: creating discrete, isolated social spheres for their private lives separate from the bureaucratized economic system in which most of them work. America, he argues, is as much a cluster of distinct cultures with divergent world views as it is a centralized, consolidated nation. To the degree that it has maintained stability in modern times, it has done so in large part because the members of its various "segments" have managed to retain a certain autonomy within the larger, national culture and have thus managed to avoid the difficult and disorienting task of adapting their lives and their values to the standards of people different from themselves. "What held Americans together," he wrote, "was their ability to live apart. Society depended upon segmentation."⁶¹

Wiebe's argument, a less startling one today, in the age of multiculturalism, than it was in the 1970s, provides at least a partial explanation for America's recent cultural conflicts. Much of the history of the postwar United States has been the story of two intersecting developments. One is the survival of fundamentalist private values among people who have in other ways adapted themselves to the modern public world. The second is the unprecedentedly vigorous assault on those values by liberal, secular Americans.

and *Ethnicity in the 1960s and 1970s* (Chapel Hill, N.C., 1991). See also Alan Wolfe, "Sociology, Liberalism, and the Radical Right," *New Left Review*, 128 (July–August 1981): 3–27, for a discussion of the way sociologists have viewed the Right; and Michael Kazin, "The Grass-Roots Right: New Histories of U.S. Conservatism in the Twentieth Century," *AHR*, 97 (February 1992): 136–55, for a perceptive overview of recent historical and sociological scholarship. Recent non-scholarly books that have tried to treat the populist Right as a political force with rational grievances include Thomas Byrne Edsall with Mary D. Edsall, *Chain Reaction: The Impact of Race, Rights, and Taxes on American Politics* (New York, 1991); and E. J. Dionne, Jr., *Why Americans Hate Politics* (New York, 1991).

⁶¹ Robert H. Wiebe, *The Segmented Society: An Introduction to the Meaning of America* (New York, 1975), 46.

To many liberal intellectuals in the 1950s and 1960s, nothing was so alluring as the ideal of what David Hollinger and others have called "cosmopolitanism," an outlook that stressed the virtues of tolerance, relativism, and rationalism and that was generally accompanied by a strong contempt for what liberals considered the backward "provincial" mind, with its presumed superstitions and prejudices. The cosmopolitan creed argued that "provincialism" (religious provincialism, ethnic provincialism, regional provincialism) accounted for the survival of racism and bigotry; provincialism stood in the way of progress and rationalism. A culturally segmented America was no longer acceptable; only by universalizing the values of cosmopolitanism, by launching an assault on the backwardness and intolerance and anti-rationalism of the "village mind," could the United States become a truly enlightened society worthy of serving as a model to the world.⁶²

The politics of the 1980s and 1990s suggest that this effort—the effort to make the values and assumptions of liberal, secular Americans the values of all Americans—has failed. It has contributed to some great accomplishments, to be sure: most notably in loosening the grip of racism and sexism on American life. But it has not eliminated, and has in many ways increased, the cultural chasms separating different groups of Americans from each other. Members of the secular center continue to define America as a society committed to modern rationalism, free inquiry, scientific discourse, and above all progress. But members of the fundamentalist Right continue, despite (or perhaps because of) the assaults of recent years, to define America as a very different society: as a bastion of traditional (or "family") values and traditional faith in an increasingly godless age; as a citadel of righteousness in a corrupt world; as the earth's only truly Christian nation. It is unthinkable for secular Americans to contemplate any retreat from the rational, progressive course on which they have long assumed the nation is irrevocably embarked. But it is equally unthinkable for fundamentalists to consider abandoning in the name of progress the values and faiths that give their lives meaning and their communities definition.⁶³

It has not been easy or comforting for liberal, secular Americans to assume (as many have done) that the fundamentalist Right is an irrational, rootless "lunatic fringe," plagued by cultural and psychological maladjustments. But it may be even more difficult and less comforting for secular intellectuals (and hence for most historians) to accept that fundamentalists can be rational, stable, intelligent people with a world view radically different from their own. For to accept that is to concede that they may have been wrong in some of their most basic assumptions about America in our time. It is to recognize that the progressive modernism that most scholars, and many others, have so complacently assumed has become firmly and unassailably established in America—the secularism, the relativism, the

⁶² See, for example, David A. Hollinger, "Ethnic Diversity, Cosmopolitanism and the Emergence of the American Liberal Intelligentsia," *American Quarterly*, 27 (1975): 133–51; Terry A. Cooney, "Cosmopolitan Values and the Identification of Reaction: *Partisan Review* in the 1930s," *Journal of American History*, 68 (1981): 580–98; Daniel Joseph Singal, "Beyond Consensus: Richard Hofstadter and American Historiography," *AHR*, 89 (October 1984): 976–1004.

⁶³ For an exploration of the centrality of religion to much of American life, and a critique of intellectuals, scholars, and others for attacking or ignoring it, see Stephen L. Carter, *The Culture of Disbelief: How American Law and Politics Trivialize Religious Devotion* (New York, 1993).

celebration of scientific progress—may not in fact be as firmly entrenched as they thought. It is to admit that modernism is not yet truly secure; that, even in America, some of the most elementary values and institutions of modern society still have not established full legitimacy with a large, and at times politically powerful, segment of our population.

THE “PROBLEM” OF AMERICAN CONSERVATISM as I have tried to describe it here is, in the end, a problem of historical imagination. It resembles other problems historians have encountered as they have opened up the world of scholarship and have struggled to retrieve previously unexamined communities and social experiences. But, while historians have displayed impressive powers of imagination in creating empathetic accounts of many once-obscure areas of the past, they have seldom done so in considering the character of conservative lives and ideas. That has no doubt been a result in part of a basic lack of sympathy for the Right among most scholars. But it is a result, too, of the powerful, if not always fully recognized, progressive assumptions embedded in most of the leading paradigms with which historians approach their work.

Understanding America in the twentieth century requires, ultimately, more than an appreciation of the central role of liberalism (in all its various forms) in our modern history, and more, too, than an understanding of the important role of the Left in challenging liberal claims. It requires, as well, a recognition of many alternative political traditions, including those of the Right. That will not be an easy task. Conservative traditions in America are diverse and inconsistent: both libertarian and normative, both elite and popular, both morally compelling and morally repellent. They fit neatly into no patterns of explanation with which most historians are comfortable. But scholars have redefined their categories and paradigms repeatedly in recent decades to help them understand areas of the past they had previously neglected. It may be time for us to do so again.

Will the Real Conservative Please Stand Up? or,
The Pitfalls Involved in Examining Ideological
Sympathies: A Comment on Alan Brinkley's
"Problem of American Conservatism"

SUSAN M. YOHN

I WOULD IMAGINE that conservatives reading Alan Brinkley's article would take heart from his declaration that historians have been remiss in addressing American conservatism; the article would seem to validate conservative criticisms of academics, that we favor that which is liberal, or even worse, radical. Yet the article must also be a source of frustration. Brinkley is not quite sure what constitutes conservatism. It is not, he asserts, easily characterized; there is no conservative ideology per se but rather a cluster of related ideas. It "encompasses a broad range of ideas, impulses, and constituencies," a description that sounds much like Daniel Rodgers' characterization of the Progressive movement, a presumably "liberal" movement.¹ Indeed, suggests Brinkley, conservatism has a philosophical base not significantly different from that of "liberalism." The two are rooted in the same political impulse: defense of liberty and the preservation of individual freedom. How, then, are we to differentiate between liberal and conservative? The distinction, according to Brinkley, seems to be in the conservative critique of statism and conservatives' growing concern over the erosion of what they see to be a "much-needed moral and spiritual core." These two issues, especially, have given conservatives an intellectual and political force that *had previously been missing*.

I must confess to some confusion. As a social historian interested in the development of the welfare state in this century, I had not realized that conservatives (of many different stripes) had not played a continuing and vital role in this process. Rather than respond to Brinkley's article by presenting an alternative synthesis of historical literature in which conservatives do wield power and influence—something that Leo Ribuffo does quite nicely in his comment—I would like to suggest that the "problem of historical imagination" to which Brinkley alludes requires more than simply doing a kind of compensatory history in which historians are sympathetic to their "conservative" subjects. What is required is a rethinking of the traditional ideological and political categories used

¹ Daniel Rodgers, "In Search of Progressivism," *Reviews in American History*, 10 (December 1982): 113–32.

by Brinkley, who assumes that "liberalism" and "conservatism" comprise the ideological compass by which most Americans fix their political experience. Throughout this comment, I will use the terms "liberal" and "conservative," but I am disturbed by the implication in the structure of Brinkley's argument that the American political experience fits neatly within the bounds of these terms and only these terms. The effect is to simplify what is a much more complex political contest that involves a spectrum of ideological positions and engages a growing number of Americans whose political loyalties are not as fixed, or as easily categorized, as we might wish them to be.

Characterizing American politics—both liberal and conservative—as focused on the preservation of individual freedom belies, for instance, the experience of those groups of Americans who have been largely disenfranchised. Brinkley assumes a particular relationship to the political structure, and he implies that all individuals share a political imagination bounded by a set of common principles. For women and African Americans, for example, demands for suffrage and civil rights (or the desire to be recognized as full citizens) have often served to mute distinctions between "liberal" and "conservative" (or, for that matter, "radical"). Ideological differences have been subordinated in order that a group of people may appear to make demands based on their common identity. Hence women demand the vote as women, not as liberals or conservatives, and African Americans make appeals as an oppressed race. For these groups, the struggle has been to secure individual freedom, not to preserve it.

What Brinkley suggests for guiding principles are, in these cases, reduced to differences in strategy, as different factions make alliances to argue for the political tools necessary to gain individual freedom. The suffrage movement, for instance, presented itself as a universal movement of women. Ellen DuBois has noted in her work on Harriot Stanton Blatch that suffragists proclaimed themselves "the union of women of all shades of political thought and of all ranks of society on the single issue of political enfranchisement." This is not to say that ideological differences did not have an impact on the success or longevity of social movements. DuBois argues that the suffrage movement had an "elite bias" and that it included Socialists but that leaders preferred not to "associate too closely with Socialists and lose access to the wealthy." Unwilling to address larger issues of class, the suffrage movement was able to secure the passage of the Nineteenth Amendment but could not sustain a larger feminist movement with universal appeal for women.²

Furthermore, the desire to dichotomize and to explain American politics on the basis of these categories of "liberal" and "conservative" may serve to obscure the very nature of the struggle people engaged in. Some conservatives and liberals have defined themselves in opposition to each other, but other movements have made claims to a unity that transcended these differences. Suffragists played down ideological differences precisely because they understood that the strategy of their opponents was to divide them, to convince women that they did not have

² Ellen C. DuBois, "Working Women, Class Relations, and Suffrage Militance: Harriot Stanton Blatch and the New York Woman Suffrage Movement, 1894–1909," *Journal of American History*, 74 (June 1987): 57.

a mutual interest. Amy Swerdlow, in her work on the Women's Strike for Peace, notes that the women who fought against the arms race claimed to have brought together a group drawn from "all political persuasions." Called to appear before the House Un-American Activities Committee, they resisted what Swerdlow calls the "demonology of the cold war," or the committee's attempt to divide their ranks, by appealing to a sense of "sisterhood."³

By seeking to categorize political action and thought, to make a divide between "liberal" and "conservative," one also risks not hearing other voices. I am thinking, for example, of Evelyn Brooks Higginbotham's work on black churchwomen. Higginbotham notes that the history of black women in the Baptist church raises questions about the "authorial" voices in the lives of "ordinary black people." Contending that the accommodation-versus-protest dichotomy, the Booker T. Washington versus W. E. B. Du Bois debate (cast in many classrooms as a debate between conservative and liberal) has for too long dominated studies, Higginbotham suggests instead that Nannie Burroughs was a more popular and charismatic leader of African Americans. Burroughs, and the movement of which she was a part, is difficult to categorize ideologically. Black Baptist women never accepted the legitimacy of segregation. Before demanding the end of segregation, however, they did attempt to "hold white America accountable for 'equal,' though separate, accommodations."⁴

To understand American political history as being primarily a contest between liberals and conservatives ultimately masks the complexity of political experience. As an intellectual, I use "conservative" and "liberal," among other terms, to make sense of the American political experience. But, as a social historian, I must confront the fact that these are not terms that the religious women I have studied applied to themselves. As I read Brinkley's article, I asked myself whether his representations of "conservative" and "liberal" are shared by most people or whether these terms do not obscure shades of political activism and subtleties of political self-consciousness. For many Americans, ideological distinctions are not as clear as this. Individuals are many things, they occupy many different roles, and, as social historians have pointed out, political ideas are shaped by crosscurrents of social forces determined by race, gender, and class, among other things. The problem for the historian is not simply to represent what is "conservative" in this society but to discuss American lives in a way that gives full expression to the forces and ideas shaping individuals.

When I first began my research on Protestant women home missionaries in the Southwest, I expected to be studying a movement that was essentially conservative in its orientation. The women's home mission movement that emerged in the latter part of the nineteenth century promised to "save" the United States from a variety of evils, including "paganism," "communism," "nihilism," and, worst of all, "Romanism." A determination on the part of the movement's leaders to wed evangelical religious principles to an expanded public role attracted thousands of

³ Amy Swerdlow, "Ladies' Day at the Capitol: Women Strike for Peace versus HUAC," *Unequal Sisters: A Multi-Cultural Reader in U.S. Women's History*, Ellen C. DuBois and Vicki Ruiz, eds. (New York, 1990), 404.

⁴ Evelyn Brooks Higginbotham, *Righteous Discontent: The Women's Movement in the Black Baptist Church, 1880-1920* (Cambridge, Mass., 1993), 221-29.

adherents. However, this movement—if conservative in origin—proved harder to categorize over time. In fact, I concluded that the movement, largely because of the work performed by home missionaries, helped to build the foundation for the “liberal” welfare state that emerged in the twentieth century.⁵

Why had I originally believed that the goals of the home mission movement were “conservative”? its embrace of Protestant evangelicalism (even though, in the late nineteenth century, this was not necessarily “conservative”) and its assumption that our democracy rested on a Protestant foundation, that the expansion of Catholicism, for instance, would corrupt the national interest. There were other indicators as well: the focus on both individual morality and the process of conversion, the suggestion that social problems were the result of sinful and immoral behavior on the part of individuals. And, perhaps most important, the xenophobia that characterized the movement: the anti-Catholicism and the fear of immigrants and other non-Anglo groups already living in the United States.

By 1930, the goals of the movement were substantially altered. The religious mission with its emphasis on conversion had given way to a greater concern with social justice. Missionaries recognized that, in the face of changing social, political, and economic circumstances, spirituality was only part of a solution to the ills that plagued their clients. In New Mexico, for instance, Hispanics were losing their lands and, in the process, becoming impoverished. Religious education thus gave way to community centers and hospitals. The more secular emphasis allowed for joint projects with groups that had formerly been suspect—such as the Catholic church. Between the 1870s and the 1920s, the women’s home mission movement became “liberalized.” Confirmation of this change can be found in the Presbyterian church, where the Woman’s Board of Home Missions came under scrutiny from fundamentalists in the 1920s for its growing emphasis on the social gospel. The purpose of the movement, argued critics, was to convert people, to minister to their spiritual needs. All other activities were social work, better left to secular social service agencies.

Were the women active in this movement “conservative” or “liberal”? Should the home mission movement be classified “liberal” or “conservative”? More important perhaps than any label are the specific policies and actions of the organization. A history of this movement points up some of the same features that Brinkley finds recurring in his study of American liberalism. For, as the women’s home mission movement allied itself with the progressive state, it suffered from some of the same shortcomings: a growing emphasis on expertise that undercut what had been the lifeblood of the organization—the volunteer—and an increasingly bureaucratic structure that alienated workers. I have concluded that these missionaries were “reluctant liberals.” They supported the creation of public programs that aided mission endeavors, yet they remained skeptical about the state’s ability to solve the problems of their clients. They never completely rejected the earlier “conservative” emphasis on individual responsibility and morality, nor did they repudiate the evangelical impulse. These beliefs remained important in

⁵ Susan M. Yohn, *A Contest of Faiths: Protestant Women Missionaries, Evangelism, and Liberal Reform in the Hispano Southwest, 1867–1924* (Ithaca, N.Y., forthcoming).

motivating the women to do mission work even when, in their work in the field, they ceased to emphasize conversion.

The home missionaries did not call themselves "liberals," nor did they refer to themselves as "conservative." If anything, they were suspicious of formal politics, viewing political labeling and partisan politics as male activities largely exclusive of women.⁶ And, while they may have continued to emphasize the importance of individual morality, they were not reactionary in the manner of the women studied by Kathleen Blee.⁷ The early ethnocentrism of the mission movement had given way by the twentieth century to an ethic that stressed greater tolerance of cultural difference. Missionaries may have shared a conservative concern that an enlarged state bureaucracy would threaten the informal and personal voluntarist ideals that had guided their work, but they repudiated the racism of right-wing Protestant movements such as the Klan.

If the ideological sentiments of women missionaries are hard to categorize, one would expect that Blee's study, *Women of the Klan*, would present no such confusions. Looking to conserve "100 percent Americanism," the white women who are Blee's subjects espoused a commitment to Protestantism, "traditional" social values, and were deeply ethnocentric and racist—all positions that qualify as "conservative." Yet they also shared a commitment to expanding women's participation in the Klan. In other words, they advocated an equal role for white (Anglo-Protestant) women with men in preserving the America they envisioned. Several of the leaders discussed by Blee came to the Klan as advocates of women's suffrage and the Equal Rights Amendment.⁸ In addition, it may well be that these members of the Klan supported a variety of movements with competing agendas. Blee notes tangled ties between the Klan and Protestant denominations, in which, for example, the Presbyterian Ladies Aid Society would serve refreshments at Klan rallies—the very same Aid Society, perhaps, that sent annual collections to the Presbyterian Woman's Board of Home Missions to support the schools run by missionaries for non-whites and non-Protestants.⁹ I mention this contradiction to suggest how difficult it can be to tease out the ideological sentiments of those active in social movements.¹⁰ Blee's Klanswomen were not consistently "conservative." What they remembered when interviewed many years later was not simply that they worked to promote certain ideals but that, perhaps more important, the Klan was a "way to get together and enjoy." It is the many inconsistencies and contradictions about this movement that make it interesting

⁶ Their experience resembles that described by Paula Baker, "The Domestication of Politics: Women and American Political Society, 1780–1920," *AHR*, 89 (June 1984): 620–47. Excluded from the partisan political culture of men, women missionaries, like their secular counterparts, shifted their focus instead to social policy.

⁷ Kathleen M. Blee, *Women of the Klan: Racism and Gender in the 1920s* (Berkeley, Calif., 1991).

⁸ See in particular Blee's discussion of Klan leaders Alma Bridwell White and Robbie Gill, *Women of the Klan*, 74–75 and 53.

⁹ Blee, *Women of the Klan*, 167.

¹⁰ Other women's organizations or groups—not right-wing—also evidenced a similar tangle or confusion of ideological sentiment in the 1920s. Women active in both "feminist" and peace organizations during this period often found themselves pulled by competing interests and loyalties. See, for example, Nancy Cott, *The Grounding of Modern Feminism* (New Haven, Conn., 1987), chap. 8; see also Joan M. Jensen, "All Pink Sisters: The War Department and the Feminist Movement in the 1920's," in *Decades of Discontent*, Lois Scharf and Joan M. Jensen, eds. (Boston, 1987), 199–222.

and that lead Blee to comment in her introduction about the women she interviewed, "[D]espite my prediction that we would experience each other as completely foreign . . . I shared the assumptions and opinions of my informants on a number of topics (excluding, of course, race, religion and most political topics)." Contrary to her expectations, she did not find these people "strange" or "repellant."¹¹

Blee's analysis of women in the Klan is the kind of work that Alan Brinkley would like to see more of; it is an empathetic, yet critical, examination of the lives and ideas of one group of conservatives. In fact, as recent review articles have noted, the revisionism for which Brinkley calls has been under way for some time. In their reviews of new literature, both Michael Kazin and Leo Ribuffo have pointed out how the new work reveals the constancy of conservative movements and shows them to be neither "extreme" nor a "reaction."¹² Furthermore, these reviewers also suggest that the conservative emphasis on morality and religion and conservatives' seeming antipathy to statism—what Brinkley sees as the distinguishing features of the conservative philosophy—are more rhetorical than real. Ribuffo, for example, reminds us that fundamentalists and conservative evangelicals (who represent themselves as steadfast advocates of "traditional values" who are excluded from the mainstream by their secular counterparts) have retained connections to the political mainstream throughout the twentieth century. From the Scopes trial in 1925, to the legislation in 1954 that added the phrase "under God" to the Pledge of Allegiance, to Billy Graham's endorsement of Richard Nixon, to the most recent debates about the content of school curriculums, religious conservatives have long ensured that their conception of morality played a role in defining the political debates of the day. Kazin cautions that conservative conceptions of "traditional values" are evolving, not static, but they have nevertheless served to "mobilize a variety of discontents under a consensual rubric that invokes both order and altruism, liberty and community."¹³

These are not movements that are rendered less powerful in the face of liberalism. But, ironically, by constructing an argument that liberalism and conservatism constitute a political dynamic in which liberalism is progressive and conservatism reactionary, Brinkley implies that conservatism has had less of an impact on the political life of the nation. Take, for instance, Brinkley's suggestion that conservatives are anti-state. It is true that the object of the current conservative animus—the glue holding together the various factions of conservative thought and impulse—has been the rise of the welfare state. Yet contemporary conservatives have been largely unsuccessful in their attempts to scale down the state, a failure that calls into question their desire to do so. Those "westerners" to whom conservatism supposedly has a special appeal may rhetorically oppose the state, but they have also welcomed government aid in a variety of forms, not just the pragmatic programs Brinkley mentions. Nor is the West monolithic or

¹¹ Blee, *Women of the Klan*, 5–6.

¹² See Michael Kazin, "The Grass-Roots Right: New Histories of U.S. Conservatism in the Twentieth Century," *AHR*, 97 (February 1992): 136–55; and Leo Ribuffo, "God and Contemporary Politics," *Journal of American History*, 79 (March 1993): 1515–33.

¹³ Ribuffo, "God and Contemporary Politics," 1516–22; Kazin, "Grass-Roots Right," 153.

unchanging in its ideological sympathies. For every "conservative" who sings the praises of rugged individualism, there is a westerner (including home missionaries) who has urged the state to address issues of social justice. Indeed, westerners have proven especially adept at changing course quickly when the need arises. In early June 1933, Wyoming was proudly proclaiming itself the only state that had not asked for or received any federal assistance. By the end of the same month, Wyoming accepted its first federal relief check. Between 1933 and 1937, per capita relief expenditures in Wyoming were double the average for the United States.¹⁴

More irksome to conservatives than the welfare state per se are the perceived beneficiaries of the state. The conservative desire that there be an identifiable "moral and spiritual core" to the nation demands a test of "worthiness" that sets limits on access to the state's services. The significance of the demand that the state support "traditional" values is not so much in the "values" being championed but rather in the idea that this "traditional" state would punish by exclusion people who do not adhere to these values. With the embracing of a Judeo-Christian ecumenism in this century, parts of the test have, of necessity, been rewritten. Disdainful of secularism, conservatives have nevertheless felt its influence; no longer is it, for example, a requirement that one be Protestant to enjoy the full rights of citizenship. The definition of "traditional values" has also become more ecumenical over the course of the century. In the 1920s, the Klan identified traditional values with "Nordic" and Protestant Americans; African Americans, Catholics, and Jews were defined as outside the state. Contemporary conservatives are more inclusive; they focus instead on "lifestyle" rather than on ethnicity (even though the underlying racism is evident in the way these "lifestyle" issues are discussed; the phrase "welfare queen," for instance, is used to delegitimize the lives and concerns of black women, despite the fact that the majority of women on welfare are white). Conservatives would punish single mothers for not marrying and having no job by limiting the benefits they could receive through Aid to Families with Dependent Children. And gays and lesbians who were discriminated against could not sue for equal protection under the law.¹⁵

This concern with the importance of maintaining a "moral and spiritual core" may be an issue raised and voiced by conservatives, but it has also determined the course of many of the "liberal" political and social movements of the century. Those of us who study what Brinkley might label "liberal" or even "radical" social movements have addressed the "problem of conservatism" in a myriad of ways—particularly studying how the goals and strategies of various social justice movements are shaped and mediated by "conservative" influence. To return to my earlier examples: appealing for equal rights, women and African Americans embraced strategies that spoke directly to the issues of morality raised by conservative critics. Suffrage, advocates argued, would allow women voters to

¹⁴ Patricia Nelson Limerick, in *The Legacy of Conquest: The Unbroken Past of the American West* (New York, 1987), provides a good introduction to the contested turf that is the American West. See esp. her chapter "Denial and Dependence," 87.

¹⁵ Recent proposals to reform welfare would limit the number of months a woman can receive benefits, and the victory of right-wing forces in Colorado in 1992 excludes gays and lesbians from anti-discrimination laws.

bring the concerns and skills they had acquired as housekeepers and spiritual guardians of the home to the public realm. Their votes would help enact legislation to fight social "evils" (such as liquor interests) and otherwise clean up a corrupt male political process.¹⁶ By embracing the politics of "respectability," African-American women sought to demonstrate their right to equality.¹⁷ As Higginbotham argues, black churchwomen "felt certain that 'respectable' behavior in public would earn their people a measure of esteem from white America."¹⁸ As my previous examples suggest, it has not been enough to argue for individual freedom or for equal access of all individuals to the same rights, regardless of what they intend to do with those rights. Individuals have had to prove their worthiness. To this end, suffragists set aside an older argument that women should have the vote because justice required it, and black women pursued a standard of respectability even though it meant that they, as Higginbotham notes, "disavowed and opposed" the culture of many poor, uneducated black men and women. The extent to which we accept and retain these kinds of tests is testimony to the continuing influence of "conservative" ideals in shaping the political life of the nation.

Brinkley illuminates the existence of conservative ideas throughout the twentieth century. What seems to be new for Brinkley, and motivates his article, is the apparently newfound success of conservatives in influencing national policy. Although Brinkley's work on leading conservative figures during the New Deal demonstrates his understanding of the power that conservatives have wielded, his article might lead one to conclude erroneously that conservatives have wielded power only at isolated moments. I applaud Brinkley's attempt to illustrate the complexity of conservatism in America and his urging of historians to pay greater attention to the many kinds of conservatism. However, we do ourselves a disservice by simply contrasting conservatism and liberalism. It reinforces the notion that conservatism is a reaction to a liberal norm that sets the terms of the debate (something Brinkley wants us to get away from doing). Instead, we would do better analytically if we explored the range of social and political forces and values that shape individuals and, ultimately, public policy.¹⁹

¹⁶ I am thinking here of those who made what Aileen Kraditor termed an "argument from expediency" for woman's suffrage. See Kraditor's articles "The Rationale of Antisuffragism" and "Two Types of Suffragist Argument," in *The Ideas of the Woman Suffrage Movement, 1890-1920* (New York, 1965), 14-42 and 55-74.

¹⁷ Higginbotham, *Righteous Discontent*, 185-229.

¹⁸ Higginbotham, *Righteous Discontent*, 14.

¹⁹ Linda Gordon and Theda Skocpol have set an example for this more expanded and comprehensive kind of analysis in their respective works on the development of the welfare state. They shift the focus away from the more traditional political categories—such as liberal and conservative—to suggest that gender and the values associated with gender, such as maternalism and paternalism, played a major role in determining the type and extent of social services enacted. Gordon has also explored the role that race played in this drama and argues that black women and white women had different visions of welfare. See Linda Gordon, "Social Insurance and Public Assistance: The Influence of Gender in Welfare Thought in the United States, 1890-1935," *AHR*, 97 (February 1992): 19-54; and "Black and White Visions of Welfare: Women's Welfare Activism, 1890-1945," *Journal of American History*, 78 (September 1991): 559-90; Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge, Mass., 1992).

Why Is There So Much Conservatism in the United States
and Why Do So Few Historians Know Anything about It?

LEO P. RIBUFFO

ALAN BRINKLEY'S ESSAY BELONGS TO A GENRE that might be called the certification narrative. From time to time, historians of high standing take (often belated) notice of research and rethinking being pursued in the ranks and certify its legitimacy, perhaps even its importance. Men and women under fifty can remember the days two decades ago when intellectual gatekeepers noticed that, yes indeed, the Cold War had something to do with capitalism and, believe it or not, Progressive Era reformers worried as much about sex and race as about rancid meat and management of the money supply.

Along with many other certification narratives, Brinkley's teeters on a paradox. Although he calls the history of American conservatism an "orphan," his own essay essentially summarizes a large body of work by other historians. Indeed, because this *AHR Forum* is likely to serve as a standard "review of the literature," we need to recognize at the outset that scholarship on American conservatism and the Far Right is both more extensive and better than he suggests.

Some omissions are especially surprising. Among intellectual historians, Allen Guttman and Ronald Lora have charted important connections between nineteenth and twentieth-century versions of conservatism; John P. Diggins, J. David Hoeveler, Jr., and Gary Dorrien have astutely analyzed the world views of major conservative and neoconservative thinkers; David Green has traced the development of "conservative" as both an epithet and a badge of honor; and Edward A. Purcell, Jr., has rediscovered the philosophical "absolutists," many of them Roman Catholics, whose arguments challenged relativism and radicalism during the Great Depression.¹ Without endorsing Arthur M. Schlesinger, Jr.'s cyclical theory of politics, we can nonetheless acknowledge his argument in book after

¹ Allen Guttman, *The Conservative Tradition in America* (New York, 1967); Ronald Lora, *Conservative Minds in America* (Chicago, 1971); John P. Diggins, *Up from Communism: Conservative Odysseys in American Intellectual History* (New York, 1975); J. David Hoeveler, Jr., *Watch on the Right: Conservative Intellectuals in the Reagan Era* (Madison, Wis., 1991); David Green, *The Language of Politics: Shaping Political Consciousness from McKinley to Reagan* (1987; Ithaca, N.Y., 1992); Gary J. Dorrien, *The Neoconservative Mind: Politics, Culture, and the War of Ideology* (Philadelphia, 1993); and Edward A. Purcell, Jr., *The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value* (Lexington, Ky., 1973).

book that conservatives have always been powerful and sometimes dominant in American history.²

Impressive biographies have been written of this century's premier Protestant agitator and anti-Semite, Gerald L. K. Smith; conservative senator, Robert A. Taft; old progressive critic of the New Deal, Herbert Hoover; countersubversive bureaucrat, J. Edgar Hoover; and the founding father and then the faltering executor of the most successful center-right coalition, Dwight D. Eisenhower and Richard M. Nixon.³ Diplomatic historians Wayne S. Cole, Justus D. Doenecke, Ronald Radosh, and Geoffrey S. Smith have provided guides to conservative and Far Right participants in the broad noninterventionist coalition before World War II.⁴ No competent historian or sociologist of American religion found the New Christian Right "baffling." Sensible analysts put this movement in historical context from the outset, and the scholarship in several disciplines is now enormous.⁵ With the possible exception of civil rights, no post-World War II domestic issue has received more scholarly attention than this century's second Red Scare.⁶

Such omissions aside, Brinkley unfortunately departs from his customary judiciousness in evaluating the earlier work by "pluralist" social scientists and consensus historians. Daniel Bell, Richard Hofstadter, and Seymour Martin Lipset were wrong about the Right in ways that have been documented for twenty-five years. They discounted nativist beliefs and conspiratorial inclinations within the political elite and ideological mainstream, underestimated the disruptiveness of social change, exaggerated American society's capacity for quiet compromise, postulated the illegitimacy of cultural as well as class conflict, reflexively celebrated the political center, regularly reduced dissident ideas to psychological symptoms, and often conflated the American Far Right with Italian

² For a recent statement of this theory, see Arthur M. Schlesinger, Jr., *The Cycles of American History* (Boston, 1986), 23–48.

³ Glen Jeansonne, *Gerald L. K. Smith: Minister of Hate* (New Haven, Conn., 1988); James T. Patterson, *Mr. Republican: A Biography of Robert A. Taft* (Boston, 1972); Richard Gid Powers, *Secrecy and Power: The Life of J. Edgar Hoover* (New York, 1987); Athan G. Theoharis and John Stuart Cox, *The Boss: J. Edgar Hoover and the Great American Inquisition* (Philadelphia, 1988); George H. Nash, *The Life of Herbert Hoover* (New York, 1988); Joan Hoff-Wilson, *Herbert Hoover: Forgotten Progressive* (Boston, 1975); Richard Norton Smith, *An Uncommon Man: The Triumph of Herbert Hoover* (New York, 1984); Stephen E. Ambrose, *Eisenhower*, Volume 1, *Soldier, General of the Army, President-Elect, 1890–1952* (New York, 1983), and Ambrose, *Eisenhower*, Volume 2, *The President* (New York, 1984); Herbert S. Parmet, *Eisenhower and the American Crusades* (New York, 1972); Stephen E. Ambrose, *Nixon*, Volume 1, *The Education of a Politician, 1913–1962* (New York, 1987), Ambrose, *Nixon*, Volume 2, *The Triumph of a Politician* (New York, 1989), and Ambrose, *Nixon*, Volume 3, *Ruin and Recovery, 1973–1990* (New York, 1991); Herbert S. Parmet, *Richard Nixon and His America* (Boston, 1990); Stanley I. Kutler, *The Wars of Watergate: The Last Crisis of Richard Nixon* (New York, 1990).

⁴ Wayne S. Cole, *Roosevelt and the Isolationists, 1932–45* (Lincoln, Neb., 1983), and Cole, *Charles A. Lindbergh and the Battle against American Intervention in World War II* (New York, 1974); Justus D. Doenecke, *Not to the Swift: The Old Isolationists in the Cold War Era* (Lewisburg, Pa., 1979), and Doenecke, ed., *In Danger Undaunted: The Anti-Interventionist Movement of 1940–1941 as Revealed in the Papers of the America First Committee* (Stanford, Calif., 1990); Ronald Radosh, *Prophets on the Right: Profiles of Conservative Critics of American Globalism* (New York, 1975); and Geoffrey S. Smith, *To Save a Nation: American "Extremism," the New Deal, and the Coming of World War II*, rev. edn. (Chicago, 1992).

⁵ For an introduction to this literature, see Leo P. Ribuffo, "God and Contemporary Politics," *Journal of American History*, 79 (March 1993): 1515–31.

⁶ For a synthesis of this scholarship that also contains an excellent bibliographical essay, see Richard M. Fried, *Nightmare in Red: The McCarthy Era in Perspective* (New York, 1990).

Fascism and German Nazism. Nonetheless, their insights should not be lost in the critique. In particular, they drew attention to nonrational and even unconscious influences on political behavior (albeit in reductionist fashion) and understood the value of examining the Right in an international framework (even though their own framework was distorted by memories of World War II).⁷

Brinkley's discussion of so-called New Left historiography is also misleading. The "New Left" label, which entered our professional vocabulary via one of the most condescending certification narratives ever to appear in this journal, conflated left liberals, Christian socialists, and diverse Marxists from several generations.⁸ As an analytical category, it deserves to be interred alongside its polemical kin "extremism," "radical Right," "paranoid style," "isolationism," and "McCarthyism." Brinkley is correct that left-of-center scholars from roughly his own generation have largely ignored the Right, although three exceptions—Michael Paul Rogin, Michael Kazin, and myself—do make it into his footnotes. However, he misses the major contribution of a previous generation on the left—those whom we might call young old leftists—who entered the profession in the late 1950s and early 1960s. To say that William Appleman Williams ignored conservatism borders on the bizarre, since he spent much of his career arguing for the significance of William McKinley, Mark Hanna, and Herbert Hoover. Williams, James Weinstein, and Martin J. Sklar used the concept of "corporate liberalism" to highlight similarities between politicians, diplomats, and business leaders whom centrist historians dubiously dichotomized into liberals and conservatives.⁹ Among other young old left historians, Gabriel Kolko reinterpreted the Progressive Era as the "triumph of conservatism," Warren Susman recovered the underlying cultural conservatism of the allegedly red decade of the "thirties," and James B. Gilbert took seriously the cultural conflicts over sex, comics, and rock and roll during the allegedly silent decade of the "fifties."¹⁰

My noting these omissions and questionable interpretations is not simply a matter of giving credit where credit is due (although that is usually one appropriate response to certification narratives). My objection is that these lapses

⁷ Daniel Bell, ed., *The Radical Right: The New American Right Expanded and Updated* (Garden City, N.Y., 1963). The essays written for the 1955 edition, *The New American Right*, are considerably more subtle and reflective than those added in 1963, by which time the interpretation had become standard and the authors eminent. The starting point for criticizing the pluralist interpretation of the Far Right remains Michael Paul Rogin, *The Intellectuals and McCarthy: The Radical Specter* (Cambridge, Mass., 1967). I offer my own critique in Leo P. Ribuffo, *The Old Christian Right: The Protestant Far Right from the Great Depression to the Cold War* (Philadelphia, 1983).

⁸ Irwin Unger, "The 'New Left' and American History: Some Recent Trends in United States Historiography," *AHR*, 72 (July 1967): 1237–63.

⁹ William Appleman Williams, *The Contours of American History* (1961; Chicago, 1966); James Weinstein, *The Corporate Ideal in the Liberal State, 1910–1918* (Boston, 1968); and Martin J. Sklar, *The United States as a Developing Country: Studies in U.S. History in the Progressive Era and the 1920s* (New York, 1992). On the arbitrary dichotomy between corporate liberals and conservatives, also see Edward D. Berkowitz and Kim McQuaid, *Creating the Welfare State: The Political Economy of Twentieth-Century Reform*, rev. edn. (Lawrence, Kan., 1992).

¹⁰ Gabriel Kolko, *The Triumph of Conservatism: A Re-interpretation of American History, 1900–1916* (New York, 1963); Warren I. Susman, "The Culture of the Thirties," in Susman, *Culture as History: The Transformation of American Society in the Twentieth Century* (New York, 1984), 150–83; James B. Gilbert, *A Cycle of Outrage: America's Reaction to the Juvenile Delinquent in the 1950s* (New York, 1986). Also see Susman's respectful treatment of "The Nature of American Conservatism," in *Culture as History*, 57–74.

prompt Brinkley not only to underestimate the impact of various kinds of conservatism but also to misunderstand why so few historians know anything about it. Good books are always welcome in any field, and in this case we would understand the United States better if we knew as much about George Wallace's followers as about Tom Hayden's. Yet the chief historiographical "problem of American conservatism" is not the absence of good scholarship but the profession's failure (in the current locution) to "mainstream" the copious good scholarship that already exists. The Right holds a historiographical place comparable to that allocated to industrial workers, African Americans, and women two decades ago. Much as these groups appeared in general accounts primarily when they joined unions, marched against segregation, and won the vote, political and cultural conservatives appear primarily when they join the Ku Klux Klan, march with demagogues in the Depression, and elect a president. Brinkley himself falls into this trap when he doubts the existence of an "effective" conservative movement before the 1970s. Much earlier, conservative ideas and movements influenced the *kind* of liberalism that developed—and vice versa.

DRAWING ON THE EXISTING SCHOLARSHIP, we can bring the Right into the historiographical mainstream without resorting to archaic categories like "political fundamentalism" or strained delineations of a timeless, "organic" conservative "tradition"—approaches favored respectively by conservative and liberal intellectuals during the mid-1950s, the last time this topic was chic. Two caveats are necessary at the outset. First, ideal types such as "conservatism" and "liberalism," no less than "Far Left" or "Far Right," encompass phenomena that vary according to time, place, circumstance, and individual temperament as political and cultural battles are won and lost, coalitions shift, combatants mellow, and issues gain or lose salience. Twentieth-century liberals and conservatives define themselves not only by competing with each other but also by their relations with allies or adversaries further left and right. Moreover, without much sense of contradiction, human beings can be, for example, simultaneously economic liberals and cultural conservatives (William Jennings Bryan in the 1920s, Harry S. Truman in the 1940s, and much of the Roman Catholic hierarchy today), segregationists and political liberals (Orval Faubus in the 1950s) or even left liberals (Claude Pepper in the 1940s), aesthetic conservatives and political liberals (Norman Rockwell in the 1950s), or aesthetic modernists and political conservatives (Hilton Kramer in the 1990s).

Second, twentieth-century American conservatism crystallized in a country that already had deeply embedded patterns of belief and behavior: a sense that the United States was uniquely blessed but also uniquely vulnerable to alien *isms*; a de facto Protestant establishment that heightened missionary diplomacy and expansion abroad as well as nativist campaigns at home; a distrust of the central government codified in the Declaration of Independence and reorchestrated by Jacksonian Democrats and late nineteenth-century agrarian rebels; a producer ethic requiring real men to make something useful in order to merit prosperity.

and real women to serve by their side; diverse regional suspicions of various metropolitan centers and the snobs who lived there; and white racism institutionalized in slavery and segregation. These patterns of belief and behavior are neither uniquely nor necessarily conservative, but they have *in fact* decisively affected the development of the twentieth-century Right.

As both the pluralists and old new leftists sensed, it is hard even in retrospect to tell a liberal from a conservative before the 1930s. It is by no means clear, for example, which major party presidential nominee, Herbert Hoover or Alfred E. Smith, was more liberal in 1928. The Depression and New Deal created a relatively clear political spectrum as well as a powerful political vocabulary that survived, albeit with significant modifications, for six decades. Proponents of the welfare state usually belonged to the Democratic Party and, following Franklin D. Roosevelt's linguistic lead, increasingly called themselves liberals. To the left of the New Deal, Socialists and Communists sought a more extensive welfare state in the present and aspired to abolish capitalism in the future; between 1935 and 1939, Communists proclaimed a Popular Front and gave de facto support to the New Deal. Far Right groups, often led by such theologically conservative clergy as Father Charles Coughlin, Rev. Gerald Winrod, and Rev. Gerald L. K. Smith, promoted a mixture of fervent nationalism, nativism, and—occasionally—economic redistribution.

Although the question of government intervention in the economy was central to this sorting out, the cultural conflicts that had been building during the previous three decades did not disappear with the crash of 1929. On the contrary, legacies from those conflicts, often combined with older American themes, influenced public debate on the welfare state more than technical economic analysis did. Some of the most powerful legacies and motifs were hospitable to conservatism. For example, the Depression did not eradicate suspicion of the central government in general or government aid to the "undeserving poor" in particular; in 1935, when roughly one-fifth of the work force was unemployed, 60 percent of Americans polled by Gallup believed relief expenditures to be too high.¹¹ Even sophisticated conservatives ritualistically attacked the New Deal as the latest scheme by government insiders to waste money and subvert American virtue. To the many conservative and Far Right heirs to the "100 percent Americanism" campaigns of the Progressive Era and "tribal twenties," the Roosevelt administration also looked subversive because it empowered men and women who seemed less than fully American: Jews, working-class Catholics, irreverent young men from the metropolis, and (occasionally) African Americans. Beyond these partisan divisions, there is much evidence to support Warren Susman's contention that American culture grew more conservative during the Depression: the decline of feminism; Hollywood's imposition of a prim production code; the emergence of neo-orthodox theology, neo-Freudian psychology, and neo-Aristotelian philosophy; and nationalistic celebrations of "the people" by filmmakers, popular novelists, and penitent expatriate intellectuals.

In this context, Franklin Roosevelt succeeded politically and the Democrats

¹¹ Poll figures are from John M. Allswang, *The New Deal and American Politics: A Study in Political Change* (New York, 1978), 114.

established themselves as the majority party by combining a rudimentary welfare state with sufficient cultural conservatism to deflect charges that the New Deal was un-American. For all of his patrician insouciance, the president never doubted that God blessed America nor did he hesitate to ask publicly for divine guidance during national crises. FDR was no card-carrying member of the American Civil Liberties Union; a rhetorical campaign against free-lance gangsters and organized criminals was launched to enhance his administration's popularity during the first Hundred Days, and J. Edgar Hoover's countersubversive campaigns subsequently prospered under his patronage.

According to a convention widely honored by politicians, pundits, and the few historians still interested in presidents, liberal administrations produce liberal legislation, and conservative administrations produce conservative legislation. The reality is always more complicated, and never more so than during the 1930s, when the Roosevelt coalition stretched for a time from Wilsonian segregationists to Popular Front liberals. Mobilizations on the left facilitated passage of important New Deal legislation, as historians now routinely acknowledge, but pressure from the right was at least as significant in shaping presidential priorities, the scope of programs, and the success or failure of implementation. The Works Progress Administration was a monument to the producer ethic. Measures as diverse as agricultural subsidies and Aid to Families with Dependent Children honored the shibboleth of local control. Yielding to politically or culturally conservative adversaries or allies as well as his own caution, Roosevelt stood aloof from anti-lynching legislation, distanced himself from the Spanish Republic, and agreed to cuts in relief expenditures that precipitated a recession starting in 1937.

World War II and the Cold War rendered American culture more restrictive, increased the power of political conservatives, narrowed the range of political debate, and moved liberalism itself rightward. By the time the United States entered World War II, Roosevelt had expelled from the ranks of legitimate liberalism those proponents of the welfare state who still supported an "isolationist" foreign policy and welcomed into his coalition internationalists who had formerly believed, or professed to believe, that the New Deal was an alien import. Several of these internationalist conservatives, including Henry Stimson, Dean Acheson, James Forrestal, and John J. McCloy, helped to manage wartime mobilization and remained present at the creation of the Cold War. Congressional conservatives gained ground as the war brought both prosperity and fear of disorder, typified in the latter case by widespread denunciations of sexually active adolescents, assertive African Americans, and "Amazon" women on the assembly line. Roosevelt swerved both left and right in 1944 and, in his most consequential rightward swerve, replaced Henry Wallace with Harry Truman as his running mate.

Although Truman was an economic and political liberal, his victory in 1948 derived partly from his public persona as an unpretentious middle American whose Missouri twang contrasted favorably with Thomas E. Dewey's metropolitan "perfect diction." Moreover, political conservatives prevented passage of most Fair Deal legislation, placed major restrictions on organized labor, and pulled liberalism further rightward. The exact proportions of Machiavellianism and morality in Truman's Cold War liberalism remain in dispute, but the hope of

outflanking critics on his right certainly influenced his decisions to create a loyalty-security program and expand the Korean War north of the thirty-eighth parallel.

The impact of various kinds of conservatism would be more evident if historians paid closer attention to the underlying insecurities and cultural conflicts of the "fifties" instead of treating them, if at all, as precursors of the "sixties." In 1956, the young old left sociologist C. Wright Mills argued that a "conservative mood" dominated American life, and his insight can be applied to the whole social-cultural "decade" stretching from roughly 1947–1948 to 1965–1966.¹² Manifestations of this mood reveal worries that disorder might break through the celebrated veneer of certainty and civility: a religious revival that particularly strengthened theologically conservative—even apocalyptic—faiths; local Red Scares that persisted long after Senator Joseph R. McCarthy was censured in 1954; denunciations of public schools for allegedly leaving Johnny unable to read while Ivan studied rockets in his Soviet sandbox; assaults on insidious "mass culture" by former radicals no longer enamored of "the people"; congressional investigations of ethnic mobsters, juvenile delinquents, and labor racketeers whose activities were taken to symbolize broader national decay—hearings that advanced the careers of four future presidential contenders, Estes Kefauver, John F. Kennedy, Barry Goldwater, and Robert F. Kennedy. Although President Eisenhower preferred the "middle of the road," his was a center-right coalition, and when Eisenhower swerved right he exploited the continuing Red Scare, experimented with a white "southern strategy," and saw socialism creeping beneath liberal plans to expand the welfare state.

When Eisenhower swerved left, he was attacked for losing the Cold War and accepting a "dime store New Deal." Some of his attackers, including Russell Kirk and William F. Buckley, Jr., received much more serious attention than Brinkley suggests; for example, *Time* devoted an entire book review section to Kirk's study *The Conservative Mind*.¹³ Such self-consciously "new conservatives" need to be placed in the broader context of mainstream politics. Like the more numerous centrist liberals, they were clearing their vulnerable flank and searching for a charismatic leader. Specifically, while centrists expelled Popular Front liberals and "anti-anti-Communists" from the "vital center," the new conservatives repudiated the "isolationists" and anti-Semites who had been *their* unseemly allies during the 1930s. Their rising charismatic leader, Senator Barry Goldwater, may have looked like a Robert Taft with glamour, as reporters often described him, but he lacked Taft's qualms about intervention abroad. This parallel ideological restructuring by movements that remained, through a process of stringent elimination, the only respectable Left and Right, pushed the whole political spectrum rightward. John F. Kennedy, who established himself as liberalism's charismatic leader in 1960, courted big business, distanced himself from civil rights militants, and deflected hostility on the campaign trail by telling stories of the Senate rackets committee. Although the exact proportions of Machiavellianism and morality in

¹² C. Wright Mills, *The Power Elite* (New York, 1956), 325–42.

¹³ For the reception of Kirk's book, see Anne Carson Daly, "The Conservative Mind at Forty," *Intercollegiate Review*, 29 (Fall 1993): 46–47.

Kennedy's Cold War liberalism also remain in dispute, his policies toward Cuba and Vietnam were certainly affected by his wariness of a conservative backlash.

The impact of various kinds of conservatism would also be more apparent if historians acknowledged that the "sixties" was not a radical era but a polarized era. Movements to expand the welfare state, end the Vietnam War, foster secularization, legalize abortion, and secure equal rights for blacks, women, and gays produced opposite and, for a time, at least equal reactions.¹⁴ In 1964, a grass-roots mobilization as significant as any on the left propelled Barry Goldwater to the Republican presidential nomination.¹⁵ Even after Goldwater's overwhelming defeat, conservatives remained powerful enough to alter Great Society legislation, limit the War on Poverty, and influence President Lyndon B. Johnson's decision to escalate the Vietnam War. While winning 9.9 million votes in 1968, Far Right presidential candidate George Wallace inadvertently helped Richard Nixon to position himself as a centrist. Although Nixon's center-right administration resembled Eisenhower's in many ways, his rightward swerves were more drastic. Indeed, the Watergate scandal represented both the last great cultural clash of the "sixties" and this century's third Red Scare, a scare marked by classic invocations of American mission, attacks on metropolitan sophistication, and even a touch of anti-Semitism.

A post-Watergate craving to believe in a moral America facilitated the election of Jimmy Carter, who became the most conservative Democratic president, relative to the rest of the political spectrum, since Woodrow Wilson and perhaps since Grover Cleveland. Whereas most congressional Democrats still wanted to expand the welfare state, Carter's brand of liberalism was reminiscent of pre-World War I progressivism. As a latter-day progressive, Carter promoted efficiency rather than economic equality, rhetorically repudiated interest-group politics in the name of the common good, and tried to raise the nation's moral tone. Among the many ironies of Carter's misunderstood presidency is that he legitimated issues Ronald Reagan later turned against him, including calls to limit federal "bureaucracy," cut social spending, and increase the defense budget.¹⁶

Reagan's administration represented change as well as continuity on the right. On the one hand, appropriating venerable American or conservative themes, he criticized metropolitan snobs for mocking "traditional" values and doubting America's unique mission, indicted Washington insiders for stifling individual initiative and subsidizing the undeserving poor, chastised the undeserving poor for seeking those subsidies, and accused weak-willed Democrats of losing the Cold War. On the other hand, he gave minimal support to New Christian Right proposals to restrict abortion, fund religious schools, and restore prayer to public

¹⁴ See Kenneth J. Heineman, *Campus Wars: The Peace Movement at American State Universities in the Vietnam Era* (New York, 1993), for examples of clashes between pro-war and anti-war students.

¹⁵ For the first reliable account of the grass-roots Goldwater movement, see Mary C. Brennan, *The "Right" Side of the Sixties* (forthcoming, Chapel Hill, N.C., 1994).

¹⁶ I expand on my argument that Carter was a cultural conservative and latter-day progressive (admittedly an ideal type as fragile as any other) in Leo P. Ribuffo, "Jimmy Carter and the Ironies of American Liberalism," *Gettysburg Review*, 1 (Autumn 1988): 738-49; and "God and Jimmy Carter," in Ribuffo, *Right Center Left: Essays in American History* (New Brunswick, N.J., 1992), 214-48. For an excellent overview of the Carter administration that is largely congruent with this thesis, see Burton I. Kaufman, *The Presidency of James Earl Carter, Jr.* (Lawrence, Kan., 1993).

classrooms; avoided Robert Taft's mistake of opposing organized labor without simultaneously identifying with the common man and Barry Goldwater's mistake of overestimating hostility to the welfare state; and ultimately moved from confrontation with the Soviet Union to détente. These changes should be no surprise. Not only had twentieth-century conservatism adapted to liberal trends in spite of itself, but Reagan led a right-center coalition that included neoconservatives suspicious of evangelical Protestantism and Democrats favoring assistance to the deserving working class.¹⁷

Furthermore, even after the Reagan and Bush presidencies, American culture in general and right-of-center movements in particular are now less conservative than they were twenty years ago, let alone forty or sixty years ago. Most Americans still enjoy both receiving government benefits and deploring government regulations. Senators who recently defended segregation have fallen back to denunciations of affirmative action; even George Wallace, the most popular Far Right presidential candidate in U.S. history, ended his career where he began—as a more-or-less conventional Democrat. No prominent figure in the current Christian Right purports to expose an international Zionist conspiracy. As an ironic consequence of the controversy over “political correctness,” some traditionalist conservatives have become civil libertarians.

To be sure, Vice-President Dan Quayle's charge in 1992 that a “cultural elite” undermined traditional values echoed William Jennings Bryan prosecuting John T. Scopes in 1925, William F. Buckley, Jr., scolding Yale in 1951, and Spiro Agnew stumping the country in 1970. Yet both the values labeled traditional and the behavior of self-described traditionalists have changed, too. Pat Robertson occasionally dispenses advice on sex to his followers, something unimaginable for Father Coughlin, Gerald Winrod, or Gerald L. K. Smith.

In a striking but hardly unprecedented convergence, liberalism has become more conservative, relative to the rest of the political spectrum, than it was two decades ago. Even before Senator George McGovern lost to Nixon in 1972, self-consciously centrist commentators and tacticians urged the Democrats to mute their identification with federal regulation, antipoverty programs, black protest, and cultural innovation.¹⁸ Despite the defeat of Jimmy Carter and Michael Dukakis, both viscerally conservative Democrats, endorsements of this strategic turn reached a crescendo in the early 1990s, and the movement found its charismatic leader in Bill Clinton. As a “new kind of Democrat,” Clinton in 1992 celebrated the family, invoked God's blessing as comfortably as Reagan or Roosevelt, and promised to reinvent government instead of enlarging it. The

¹⁷ Michael Schaller, *Reckoning with Reagan: America and Its President in the 1980s* (New York, 1992); Garry Wills, *Reagan's America*, rev. edn. (New York, 1988); Larry Berman, ed., *Looking Back on the Reagan Presidency* (Baltimore, Md., 1990); and Lou Cannon, *President Reagan: The Role of a Lifetime* (New York, 1991).

¹⁸ For example, see Richard M. Scammon and Ben J. Wattenberg, *The Real Majority: A Surprising Examination of the State of the Union*, with new intro. (Garden City, N.Y., 1992); E. J. Dionne, Jr., *Why Americans Hate Politics* (New York, 1991); Thomas Byrne Edsall with Mary D. Edsall, *Chain Reaction: The Impact of Race, Rights, and Taxes on American Politics* (New York, 1991); David Kusnet, *Speaking American: How the Democrats Can Win in the Nineties* (New York, 1992); and David Osborne and Ted Gaebler, *Reinventing Government: How the Entrepreneurial Spirit Is Transforming the Public Sector* (Reading, Mass., 1992).

process of reinvention involved several rightward swerves during his first year in office. Specifically, Clinton made deficit reduction and the North American Free Trade Agreement his highest priorities, declared war on crime in the manner of Roosevelt and Kennedy, and, yielding to the latest congressional conservative coalition, pared legislation to stimulate the economy. The proportions of Machiavellianism and morality in Clinton's post-Cold War liberalism will undoubtedly change during the remainder of his term. Cultural liberals, almost all of them Democrats, and cultural conservatives, most of them Republicans, still disagree about education, abortion, ethnic diversity, and gay rights. In economic and foreign affairs, however, the two major parties now stand closer together than at any time since the late 1950s, and perhaps since the late 1920s.

THIS SIXTEEN-PARAGRAPH EXPERIMENT in mainstreaming the Right slights internal divisions and ignores historical contingencies. For example, if President Gerald R. Ford had won the close election of 1976, Republicans would have borne the burden of "stagflation" and liberal Democrats might have enjoyed a resurgence, even though their positions were caricatured as dangerously radical. Nonetheless, this sketch does show that during the past six decades conservatives halted or modified "liberal" legislation, helped to widen two wars, influenced debates about sex, gender, race, ethnicity, faith, subversion, and social order, and often diverted the course of liberalism.

If the Right can be so easily connected to issues historians routinely study, why do so few of them know anything about it? Although Brinkley understands that the answer lies in attitudes pervasive among intellectuals, he resumes his customary judiciousness and does not dig deep enough. A general disposition toward "cosmopolitanism" is not the problem. After all, those scholars who do study the American Right are no less cosmopolitan (in any sense of the word) than their colleagues. On the contrary, since they typically take seriously ideological positions other than their own, they may be more cosmopolitan. Still, because few students of the Right identify with the conservatives they study, there is no conservative caucus to lobby for the field in the interest-group politics of the profession.¹⁹

Even more significant is the *kind* of Left that has become institutionally dominant ("hegemonic"?) in American history and American studies. This Left bears less resemblance to the young old leftists of the late 1950s and early 1960s,

¹⁹ The closest thing to a conservative interest group revolves around *Continuity*, a journal founded in the early 1980s by a group of historians determined to "turn the profession around," as editor Paul Gottfried wrote in "Editorial Notes," *Continuity*, 11 (1987). This editorial suggests their feeling of embattlement, as do several other articles and interviews that have appeared in the magazine. But there are useful articles on diverse conservative thinkers, cultural conflict, countersubversion, and creation of the welfare state. In particular, see Stephen J. Sniegowski, "The Darrow Board and the Downfall of the NRA," *Continuity*, 14 (Spring-Fall 1990): 108-28; Burton W. Folsom, Jr., "The Scopes Trial Reconsidered," *Continuity*, 12 (Fall 1988): 103-28; and Marvin N. Olasky, "Liberal Boosterism and Conservative Distancing: Newspaper Coverage of the Chambers-Hiss Affair, 1948-1950," *Continuity*, 15 (Fall-Winter 1991). *Continuity* contributors are not necessarily conservatives, and Cold War liberals critical of revisionist diplomatic history seem especially welcome in its pages. For the record, I am an unreconstructed McGovernite.

whose books still help us to interpret conservatism, than to the Popular Front intellectuals of the 1930s, who fought hard against the Right without understanding it. Indeed, three aspects of the Popular Front sensibility are again prevalent: an obliviousness to the unanticipated consequences of human actions (what vital center liberals called a sense of irony), an irrepressible faith in progress that finds a silver lining in every defeat, and a reflexive celebration of groups deemed "progressive." Despite complaints to the contrary, there is a standard "synthesis" of American history that can be inferred from prominent monographs, journal articles, and convention sessions. In these venues, not only are groups celebrated when they play a "progressive" role, but they also tend to disappear when they don't. For example, young white ethnic workers, whose lives during the Depression are studied in loving detail, drop from view in accounts of the "fifties" (when many of them abandoned colorful neighborhoods for bland suburbs) and return only in their late middle age as a backlash backdrop for the student radicals of the "sixties." Conservative and Far Right movements do not fit easily into this de facto synthesis.

Yet Brinkley's certification narrative, like many of its predecessors, may signal a change in the professional *Zeitgeist*. Intentionally or not, his essay merges with a broader trend to reconstruct an intellectually vital center. Signs of this trend include highly publicized books by political commentator E. J. Dionne, law professor Stephen L. Carter, and philosopher Cornel West, as well as renewed interest in the ideas of John Dewey and Reinhold Niebuhr.²⁰ Prospects for a sound reconstruction are mixed. On the one hand, Dionne and Carter, along with their pluralist antecedents, tend reflexively to celebrate the political center and exaggerate American society's capacity for quiet compromise. On the other hand, both Dionne and Carter treat conservative ideas and movements as integral parts of American life.

If an emerging centrist *Zeitgeist* does prompt scholars to bring studies of the Right into the historiographical mainstream, perhaps they can improve on the pluralists in their use of international comparisons. Building on the work of Robert Wiebe and his own insightful discussion of regionalism, Brinkley argues that our "segmented society" has impeded construction of a centralized state.²¹ The point is well taken. Yet we should be wary of Wiebe's description of the United States as a "nation of almost unparalleled diversity" as well as the inferences Brinkley draws from this premise. Since the late 1960s, four other major countries (Czechoslovakia, Yugoslavia, Pakistan, and the Soviet Union) have proved to be so segmented that they fell apart, and a fifth (Nigeria) was kept together by a brutal civil war. Moreover, even though strong localist sentiments may hinder development of a centralized state, they do not necessarily preclude—on the contrary, they may promote—powerful conservative or Far Right social movements. What large European country avoided forced nationalization

²⁰ Dionne, *Why Americans Hate Politics*; Stephen L. Carter, *The Culture of Disbelief: How American Law and Politics Trivialize Religious Devotion* (New York, 1993); Cornel West, *Race Matters* (Boston, 1993); Robert B. Westbrook, *John Dewey and American Democracy* (Ithaca, N.Y., 1991); and Richard Wightman Fox, *Reinhold Niebuhr: A Biography* (New York, 1985).

²¹ Robert H. Wiebe, *The Segmented Society: An Introduction to the Meaning of America* (New York, 1975).

campaigns during the past century? These ranged from invocations of “our ancestors, the Gauls” in French schoolbooks to genocide in Nazi concentration camps. Indeed, the success of these campaigns is one reason Americans often exaggerate the homogeneity of European countries.²²

Finally, even in the absence of a strong conservative caucus to propose them, historians of the United States should seriously consider both conservative questions and conservative answers to liberal or radical questions. Such openness would especially enrich historiographical debates that have barely begun or have sunk into torpor. For instance, we might entertain the lucid conservative response to the issue of whether or not there was “social control” during the Progressive Era: of course there was, and what else should we expect when 27 million immigrants, most of them poor Catholics and Jews, entered an overwhelmingly Protestant country within two generations? Similarly, insofar as the New Deal coalition depended on diminishing ethnic differences, this growing homogeneity may have been primarily an ironic result of immigration restriction, nativist enforcement of “100 percent Americanism,” and conscription during World War I rather than the progressive result of enthusiasm for Jack Benny’s radio show and shared shopping at the A&P. And perhaps the New Deal and Great Society were historical aberrations rather than periodic turns in a Schlesingerian cycle of reform and reaction. The main course of American political and economic history in this century may stretch from William McKinley and Mark Hanna through George Bush and Nicholas Brady to Bill Clinton and Robert Rubin.

²² Unfortunately, even in what Brinkley calls the “age of multiculturalism,” the explicit and implicit comparisons made by Americanists remain largely Eurocentric. Yet, in some respects, the United States has less in common with Great Britain than with Brazil, the other major nation to retain slavery well into the nineteenth century. Furthermore, the question of why there is so much conservatism in the United States should be pursued in tandem with the more popular perennial, “Why is there no socialism in the United States?” In both cases, we need to look beyond Europe. There has been little socialism *per se* anywhere in the Western Hemisphere. Franklin Roosevelt had less in common with Leon Blum, the leader of the French Popular Front, than with Argentine President Juan Perón, a fellow creator of a volatile cross-class coalition held together by personal charisma and the rhetoric of national solidarity.

AHR Forum
Response to the Comments of Leo Ribuffo and
Susan Yohn

ALAN BRINKLEY

I SHOULD, I SUPPOSE, BE FLATTERED by Leo Ribuffo's suggestion that, as a historian "of high standing," I am in a position to "certify" the legitimacy of an important field of scholarship. But historians of conservatism do not need me to certify their work, and I would not be so condescending as to presume to do so. Scholarship on conservatism, by historians and others, is extensive, as Ribuffo correctly claims. Much of it has been around for years. Much of it has appeared, at a rapidly increasing rate, quite recently.

My purpose in writing the essay was more modest. It was not, certainly, to serve as an "intellectual gatekeeper" or to slight the many scholars who have studied American conservatism. Nor was it to provide a "standard review" of this growing body of literature. Ribuffo is quite right that there is much good scholarship on conservatism that I do not discuss or cite, and he is good enough to bring some of it to the attention of readers of this *Forum*. My purpose was to ask why most of us do not take more notice of that scholarship and of the conservative ideas and actions it describes—ideas and actions that are central to modern American history but not yet central to the way most of us write and talk about that history. That lack of centrality should be clear to anyone attentive to (among other things) the composition of programs at professional meetings, the array of articles and reviews in professional journals, the range of courses offered in history departments, or the contents of standard American history textbooks (including my own).

I was trying, in short, to explain precisely what Ribuffo himself laments as the profession's failure to "mainstream" the study of conservatism. That is, as he says, in part a failure to take sufficient notice of the "copious good scholarship" that already exists. It is also, I would argue, a failure, spanning several generations, to produce as much good scholarship as the subject of conservatism deserves.

In his interesting riff through modern American history, Ribuffo makes a persuasive argument that "conservative" ideas have been constantly important, that even people and movements we generally consider "liberal" and "progressive" have embraced conservative positions on many issues. But to say that there has been much conservatism in American history is to state the obvious; no "competent historian" would likely dispute, or be surprised by, most of what Ribuffo reveals in his narrative.

What historians have neglected is not effective conservative defenses of the status quo by dominant institutions and leaders or concessions to the Right by centrist politicians. They have, rather, failed to give sufficient attention to the emergence in the twentieth century of important forms of oppositional conservatism, increasingly alienated from many of the major institutions of American society and from the cultural norms that have emerged to justify and support them. Such opposition has taken many forms; there have been many individuals who have taken right-wing positions on some issues and progressive positions on others. But the difficulty of isolating and categorizing conservatism is all the more reason to try to explain it.

Ribuffo's own important work on the Right, which I greatly admire, makes it clear that he understands this. I am puzzled, therefore, by his tendency here to cite scholarly descriptions of "corporate liberalism," which have long been central to interpretations of America in the twentieth century, as if it were part of a serious analysis of conservative opposition to the modern order. The "corporate liberal" model's conflation of liberal and conservative ideas—the implication in the work of historians such as Kolko, Weinstein, Williams, and others that there is no serious difference between liberalism and conservatism in modern America—is one of the reasons that historical scholarship has marginalized other important forms of conservatism.

In speculating about the reasons for that marginalization, I attempted to review several historical paradigms of the last fifty years and to ask why conservatism and the Right have played so small a role in them. Most historians of the last few generations have been liberals or people of the Left. Their work, and my discussion of it, naturally reflects their inclinations; they have tried to view conservatism through the prism of, and in contrast to, their own belief systems. Susan Yohn has interpreted my description of the world view of most historians as a description of the core assumptions of the whole of American society, and she expresses dismay at my "implication" that the "American political experience fits neatly within the bounds of these terms [liberal and conservative] and only these terms." I do not believe any such thing, and I do not think I implied otherwise.

Yohn has also misunderstood my discussion of the relationship between liberalism and conservatism—and my contention (which surely no one would dispute) that many conservatives, like many liberals, defend individual freedom. I do not claim that the effort to preserve individual freedom is the whole, or even the most important part, of modern conservatism (indeed, quite the contrary), just as I do not claim that liberalism and conservatism embrace the whole of the American political and social experience. Yohn's intelligent review of the ways in which many women, African Americans, and others choose forms of self-identification that do not fit neatly into a liberal-conservative dichotomy makes clear that conservative traditions are not the only ones missing from much historical scholarship.

But while I do not believe (and hope I did not suggest) that we should "understand American political history as being primarily a contest between liberals and conservatives," I do think the labels, and the contests among those who use them, are worthy of attention. Naturally, we should consider them, as

Yohn notes, as part of an exploration of “the range of social and political forces and values that shape individuals and, ultimately, public policy.” But too many Americans have believed (and continue to believe) that “conservatism” and “liberalism” mean something, that the words define their own (often inconsistent) belief systems or the belief systems of those they oppose, for us to dismiss them as subjects of analysis simply because they do not encompass the whole of historical experience.

Finally, Ribuffo and Yohn both bridle at my statement that “in the twentieth century, at least, American conservatism has also been relatively late in developing as a major intellectual or political force,” and they are right to do so. The passage considerably overstates the point I was trying to make: that powerful conservative movements, defining themselves by their opposition to what they consider the mainstream of American life, have been particularly important in the postwar era and have flourished primarily in the last twenty-five years. Conservative ideas and impulses have been important through all of American history and dominant in much of it; but the powerful oppositional movements that have become so central to contemporary American politics and culture are, I believe, relatively new—despite their many precursors. Major figures in those movements readily concede that themselves.

Whatever our disagreements, I am grateful to Leo Ribuffo and Susan Yohn for broadening and enriching the discussion of these important questions, and I am grateful to the *AHR* for providing all of us with this forum.

The Invention of the Ethnocultural Interpretation

RONALD P. FORMISANO

IN A MORE LEISURELY AGE, this article might have been subtitled "A Case Study in the Distortions and Reductions of Historians' Routines of Historiography." While the discipline needs periodic stocktakings, the expanding mass of publications has generated increasing pressure to reduce complex works to labels, and historians often lump diverse studies into "schools," with insufficient attention to complexity as it exists at particular moments or to change over time. Especially susceptible may be historiography written as prelude to the statement of one's own argument when entering contested terrain.¹ The practice of historiography as a reductionist process is interwoven with the thesis of this essay: the so-called ethnocultural interpretation of political parties and voting, especially with regard to the nineteenth-century United States, has been invented largely by the principal critics of the new political history. An "ethnocultural interpretation" was neither created nor advocated by the two historians most associated with initiating the new political history, Samuel P. Hays and especially Lee Benson.

These new political historians, in arguing that politics could be understood best by systematic analysis of the social context of politics, did not limit themselves to the study of ethnicity or religion, as was charged by some critics, but rather considered as broad a range of social variables as possible. Nor did they posit a society in which class structure had no meaning for political power. Benson and Hays advocated applying social science methods, concepts, and theories to political and social history in an effort to produce more complex and credible explanations of historical processes and events. Quantitative methods were incidental to these goals, but producing scholarship of general interest to historians and other social scientists was central.² Although the new political history has encompassed the interdisciplinary analysis of parties, legislatures, elites, power, policy, communities, subcultures, and political culture from the

Thanks for helpful criticism to Jean Baker, Richard Oestreicher, Michael Zuckerman, Darrett Rutman, to anonymous readers for the *AHR*, to University of Florida graduate students who received the arguments in this essay with proper skepticism, and especially to William G. Shade.

¹ Michael Kammen has warned that "it can be risky to categorize historians into schools, such as the 'consensus school.' Some of us fall into schools unknowingly; others are too hastily pigeonholed . . . and some of us simply change our minds, repudiating what we once believed as a natural consequence of intellectual growth." Kammen, "Historical Knowledge and Understanding," *Selvages and Biases: The Fabric of History in American Culture* (Ithaca, N.Y., 1987), 31.

² Ronald P. Formisano, "The New Political History," *International Journal of Social Education*, 1 (Autumn 1986): 5-21. Allan G. Bogue implicitly made the same argument, though with more emphasis on quantification, in "The Quest for Numeracy: Data and Methods in American Political History," *Journal of Interdisciplinary History*, 21 (Summer 1990): 89-116.

eighteenth to the twentieth centuries, the critique directed at nineteenth-century voting studies will be the focus here.

The new political history preceded "the new social history," and for a time in the early 1970s the two seemed indistinguishable. Whether studying family structure in seventeenth-century New England or political parties in the antebellum Midwest, historians prepped with some statistical skills and awareness of research design seemed to be going about the disaggregation of American history "from the bottom up" in similar fashion.³ There was resistance, to be sure, to quantitative methods, but eventually exaggeration both of their benefits and drawbacks subsided. More serious, because political historians helped challenge so fundamentally the Progressive historians' economic interpretation of political conflict, they seemed to threaten not just how history was done but why. Their borrowing of methods and concepts from a political science that was touting the blessings of "value free" research also fostered an impression that political history, unlike the new social history, was somehow an extension of the "consensus school" and shared its allegedly conservative agenda. In reality, however, new political historians were as diverse as the "congeries of groups" that James Henretta said made up the new social history.⁴ Practitioners ranged in personal politics from a Marxist Social Democrat to at least one conservative Republican, with most holding left-liberal views characteristic of the profession at large. More important, political historians emphasized conflict, not consensus, but it was cultural, ethnic, religious, and racial rather than economic conflict. Critics, especially on the left, saw the promotion of social science methods as antihumanist and misread the demotion of economic conflict in major party politics as a vote for consensus.

IT HAS BEEN OVER THIRTY YEARS since publication of the most important work of the new political history: Lee Benson's book *The Concept of Jacksonian Democracy: New York as a Test Case* (1961). In the 1960s and early 1970s, one heard works that followed in the same vein referred to as behavioral, or social science, or quantitative history.⁵ Gradually, particularly among critics, the term "ethnocultural interpretation" came into fashion, and it often came to stand for the new political history, thereby narrowing, reducing, and distorting what was intended to be open, broad, and flexible. In *The Concept of Jacksonian Democracy*, Benson indeed asserted the oft-quoted hypothesis "that at least since the 1820's when manhood suffrage became widespread, ethnic and religious differences have tended to be *relatively* the most important sources of political [that is, voting] differences." It was not just that some critics ignored the *relatively* but that those

³ This was my distinct impression when I attended a conference of mostly colonial historians at Brandeis University in 1970.

⁴ James A. Henretta, "Social History as Lived and Written," *AHR*, 84 (December 1979): 1293-95.

⁵ Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, N.J., 1961). Neither Benson nor Hays emphasized quantification or put quantitative methods to any extensive use. For them, research design was primary, and techniques, statistics, and computers secondary. Both believed that obsession with technique could be counter-productive. See, for example, Hays's review essay of four books dealing with quantification and computers, "Historical Social Research: Concept, Method, and Technique," *Journal of Interdisciplinary History*, 4 (Winter 1974): 475-82.

who quoted it correctly privileged that assertion in a way inconsistent with the book's logic and structure. Critics uniformly failed to notice that the chapter in which that statement appeared, while startlingly original in the context of political history as then written, was not central to Benson's analysis. They then ignored the later, synthetic chapters that built outward to integrate many variables and conditions.⁶

In his unsettling inquiry into the American historical profession's handling of the "objectivity question," *That Noble Dream*, Peter Novick showed that relativism emerged in the 1930s as an unsystematic critique of professional orthodoxy's objectivist ideal and not as a full-blown alternative philosophy. He then depicted how the defenders of the objectivist standard, especially during World War II and the Cold War, hammered the Beard-Becker critique into a rigid dogmatism quite unlike the nuanced, complex, and often inconsistent original ideas.⁷ In a similar process, some critics transformed the new political history from a posture of theoretical awareness and methodological self-consciousness, yielding no particular research outcomes, into a rigid hypothesis and unwitting determinism—namely, the "ethnocultural interpretation."

To complicate matters, there exist at least several understandings of the "ethnocultural interpretation." Some practitioners of the new political history have accepted the label ethnocultural. But what they mean is very different from what the critics/inventors have meant by the term.⁸ Some political historians have not bothered to accept or reject the term but have simply ignored it, even while incorporating criticism both hostile and friendly, and gotten on with their work.⁹ In contrast to the impression given by the inventors' "ethnocultural interpretation," the now middle-aged new political history has evolved and changed over the past twenty-five years.

Not all critics have reduced to caricature the interpretations emphasizing

⁶ Benson, *Concept of Jacksonian Democracy*, 165. The sentence appears in Chapter 8, "Ethnocultural Groups and Political Parties," which is bracketed by two similar chapters, 7, "Class Voting in New York," and 9, "Religious Groups and Political Parties." The later chapters are 13, "Outline for a Theory of American Voting Behavior," and 14, "Interpreting New York Voting."

⁷ Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988), 250–319.

⁸ See, for example, Robert P. Swierenga, "Ethnoreligious Political Behavior in the Mid-Nineteenth Century: Voting, Values, Cultures," in Mark A. Noll, ed., *Religion and American Politics: From the Colonial Period to the 1980s* (New York, 1990), 146.

⁹ Allan G. Bogue, in his most recent assessment of the new political history, did not mention the term "ethnocultural," barely mentioned critics, and concluded that the new political historians "have offered the profession a different and enhanced understanding of America's political past. They have pictured a multi-cultural . . . pluralist political world in which class and economic interest has been so crosscut by sociocultural group influences that the explanatory model of the progressive historians, emphasizing the class and economic determinants of political [voting] behavior, has been largely discredited." Bogue, "Quest for Numeracy," 113. Ten years before, Bogue gave somewhat more attention to the critics of the 1970s; see "The New Political History in the 1970s," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 249.

Joel H. Silbey also avoided use of the term in his recent synthesis of nineteenth-century politics, even while insisting that "economically rooted behavior was neither the only nor even usually the most influential element dividing the electorate between Whigs and Democrats. Ethnoreligious antagonisms were very strong . . . [and] America's uneasy cultural diversity promoted political conflict that the parties came to personify, articulate, and organize." "They were the political norm." Silbey, *The American Political Nation, 1838–1893* (Stanford, Calif., 1991), 160, 162–65, 167.

cultural factors in voting. Certain essays of criticism, however, have been so often and continuously cited by historians with varied interpretive interests of their own to promote that they have come to constitute what might be regarded as a self-referential canon.¹⁰ The critics have built on one another's work, often uncritically; and, as one reads recent political historiography, one gets the sense that younger historians and graduate students no longer depend on their own reading of the early works of the new political history but rather on the critics' essays.¹¹

The "canonized" essays of criticism not only did the most to invent the ethnocultural interpretation but did so largely by ignoring the direction of Hays's and Benson's scholarship. While the critics at a minimum served a heuristic function and beyond that raised valuable questions, for various reasons they have received a free ride. For almost twenty years, certain essays have been repeatedly cited as authoritatively as if they had been delivered to earth from Olympus or Sinai, free of all mortal error. Because of their influence, however, the level of misrepresentation is so egregious as to defy simple common sense.

Some examples: first, recently a book reviewer mentioned in passing "[e]thnocultural theories concerning the cause [*sic*] of the Civil War." In fact, there is no ethnocultural "theory" of the *causes* of the Civil War. Second, in 1990, Daniel Feller wrote, in the course of a mostly constructive essay of historiography, that in the hands of some proponents other than Benson the ethnocultural thesis "was broadened into a universal law of American political behavior."¹² No first-year graduate student, let alone any historian, is foolhardy enough to cast *any* generalization as a "universal law." As it happens, the citation to that statement offered my 1971 book on antebellum Michigan politics. My search of the book, however, has failed to discover where that "universal law" resides, and Feller himself must have had a problem locating it because the citation—like a dart thrown by a blindfolded man—stuck to two prosaic pages discussing the Michigan population of 1835—not a word about a universal law or even ethnocultural voting.¹³ Third, in 1982, in "Agrarians" and "Aristocrats," John Ashworth argued

¹⁰ The essays to be discussed below that I regard as constituting the canon because of the frequency of their citation are those by, in chronological order, James E. Wright (1973), Richard L. McCormick (1974), Eric Foner (1974), J. Morgan Kousser (1976), Richard B. Latner and Peter Levine (1976), Sean Wilentz (1982), and Don E. Fehrenbacher (1985). Michael A. Lebowitz, "The Jacksonians: Paradox Lost?" in Barton J. Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York, 1968), 65–89, was an early critique of Benson's *Concept* and of Marvin Meyers, *The Jacksonian Persuasion: Politics and Belief* (New York, 1960), that did not posit an "ethnocultural school" as a point of departure. Donald J. Ratcliffe, "Politics in Jacksonian Ohio: Reflections on the Ethnocultural Interpretation," *Ohio History*, 88 (Winter 1979): 5–36, is unfortunately not as well known as it should be. Although Ratcliffe began with the erroneous but common statement that "some historians and political scientists have seen ethnic factors as the *exclusive* determinants of voting behavior" (p. 5, *italics mine*), he paid unusually close attention to the interpretations that he modified or revised.

¹¹ Thus Daniel Feller's recent retrospective essay on *Concept* seemed to rely both on his own and the critics' reading/misreading of Benson. Feller, "Lee Benson and *The Concept of Jacksonian Democracy*," *Reviews in American History*, 20 (December 1992): 591–601.

¹² Dale Baum, review of Kenneth M. Stampp, *America in 1857: A Nation on the Brink* (1990), in *AHR*, 96 (December 1991): 1614; Daniel Feller, "Politics and Society: Toward a Jacksonian Synthesis," *Journal of the Early Republic*, 10 (Summer 1990): 140.

¹³ Feller, "Politics and Society," 140; Ronald P. Formisano, *The Birth of Mass Political Parties: Michigan, 1827–1861* (Princeton, N.J., 1971), 82–83.

throughout that Jacksonian politics did not suppress but, rather, expressed class conflict, and, in the final section of the book, he launched an extended critique of the "ethnocultural interpretation" by observing that Benson's book on New York State (1961) and my book on Michigan did not account for party conflict in the South, "where there was great ethnic homogeneity, little immigration and a Whig party which showed 'unwillingness to join the movements for humanitarian reform.'" Ashworth was not the first to ask, "What about the South?" and Thomas B. Alexander and his students already had argued for southern distinctiveness.¹⁴ But the question's logic as applied to books about northern states could extend to infinite the list of phenomena unaccounted for by Benson and me. This tactic might be called, in the manner of David H. Fischer, the fallacy of the separate question. Ashworth also made much of a "dichotomy . . . implied by Formisano and Benson between socio-economic and ethnocultural questions," a subject to be discussed below.¹⁵ Lastly, in 1990, a veteran critic of the new political history wrote that "the 'ethnocultural thesis' was invented by Benson, [in] *The Concept of Jacksonian Democracy*,"¹⁶ as if Benson's major concern had been promulgating that

¹⁴ John Ashworth, "Agrarians" and "Aristocrats": Party Political Ideology in the United States, 1837–1846 (London, 1983), 177–79; Thomas B. Alexander, Peggy Duckworth Elmore, Frank M. Lowery, and Mary Jane Pickens Skinner, "The Basis of Alabama's Ante-Bellum Two-Party System," *Alabama Review*, 19 (October 1966): 243–76.

¹⁵ Ashworth, "Agrarians" and "Aristocrats," 219. Ashworth overlooked the explicit discussion of this point in 1976. "Recent studies of Jacksonian voting . . . have forcibly suggested the importance of religious and ethnic variables . . . in shaping party loyalties, but they also indicate that they overlap with socioeconomic variables and act together with them"; Ronald P. Formisano, "Toward a Reorientation of Jacksonian Politics: A Review of the Literature, 1959–1975," *Journal of American History*, 58 (June 1976): 62, see also 59–61, 63–64.

¹⁶ J. Morgan Kousser, "Toward 'Total Political History': A Rational-Choice Research Program," *Journal of Interdisciplinary History*, 20 (Spring 1990): n. 17, 536.

Only one of the nine first reviews of *Concept* that I have found mentioned the phrase "ethnocultural interpretation," but in doing so Charles Sellers called it an important book that urged historians "to look to the social sciences" for aid in developing systematic explanations of voting. He mentioned Benson's emphasis on ethnocultural groups but added, "Benson is too sophisticated to propose a simple-minded 'ethnocultural interpretation' in place of a simple-minded 'economic interpretation.' Instead he moves far toward developing our first comprehensive, historically oriented theory of American voting behavior." Sellers, *AHR*, 67 (April 1962): 744–45. In the *Journal of Southern History*, 27 (1961), Roy Nichols asserted that *Concept's* primary significance was that, to understand politics, "we must explore . . . social structure . . . This study reveals more clearly than anything I have seen what concepts and methods we must use if we are ever to lift political history out of the unreal and imperceptive. It shows how much we can gain from the behavioral sciences," pp. 539–40. Richard P. McCormick, in the *Mississippi Valley Historical Review*, 48 (1961), called the book "challenging" and "ambitious," mentioned no "ethnocultural thesis," but was skeptical. Anticipating the position of another McCormick, Richard P. McCormick interpreted Benson to have argued that while Democrats and Whigs "presented real alternative choices to the electorate," yet "'real' [that is, economic] issues did not influence the electorate because the parties were not aligned on the basis of class, occupation or section," pp. 511–12. Similarly, Nathan Miller resisted accepting Benson's revision of Jacksonian Democratic liberalism and found his treatment of economic questions inadequate, yet Miller agreed with Benson "that greater attention to ethnocultural factors involved in voting must be given." *Political Science Quarterly*, 78 (1963): 280–82, quotation on 282. D. M. L. Farr, in the *Canadian Historical Review*, 43 (1963), 229–30, found its approach highly suggestive for Canadian history; and Marcus Cunliff, *History*, 47 (1962): 343–44, uncertainly emphasized its originality. One reviewer rejected the book as "an outstanding work of destructive criticism" and nothing more, Harry R. Stevens, *Indiana Magazine of History*, 57 (1961): 372–73; while Robert V. Remini seemed puzzled, *New York Historical Society Quarterly*, 46 (1962): 119–20. In a careful review, John Reed summarized various themes from the book and capped his discussion of voting theory with a quotation from Benson regarding the great "number and variety of factors that operate as determinants of voting behavior," *Pennsylvania Magazine of History and Biography*, 86 (April 1962): 224.

particular thesis. It should be evident to anyone familiar with Benson's papers, lobbying, and publications from the 1950s to the 1970s that he was crusading for a more systematic, self-conscious history. Benson was consumed with the problem of causation during this period, and *Concept* actually constituted the first volume of a projected study of public opinion and the coming of the Civil War. Given that *Concept* itself has been ignored, it may seem quixotic to plead for the relevance of Benson's other work, at least from the late 1950s and early 1960s. But since Benson has been held up as a founder and exemplar of a "school," it is appropriate to take his related work into account.¹⁷

Even a glance at Benson's four major publications from 1955 to 1961 shows the breadth of his concerns: *Merchants, Farmers, and Railroads: Railroad Regulation and New York Politics, 1850–1887* (1955), "Research Problems in American Political Historiography" (1957), *Turner and Beard: American Historical Writing Reconsidered* (1960), and *The Concept of Jacksonian Democracy* (1961). My thesis here can be sustained amply by considering *Concept* by itself, but why should historiography proceed as if a scholar's publications in one year are unrelated to what he or she published the next?

The same argument can be made with reference to the work of Hays, the other alleged inventor of the "ethnocultural interpretation," who published several essays during the early and mid-1960s of relevance here. In "The Social Analysis of American Political History, 1880–1920," for example, Hays argued for a broadening of what historians meant by political behavior and a contextualized approach to its local roots. All his writings were marked by an eclectic, anti-dogmatic tone.¹⁸ In his introduction to a collection of essays in 1980, Hays recalled how in the 1950s he became interested in voting during the late nineteenth century and then in "the ethnocultural characteristics of voters." "Far more important than the specific focus on ethnocultural factors in voting," he continued, "these inquiries added an important 'grassroots' dimension to my historical thinking." Earlier, he had been concerned principally with national policy, and he remained so, "but now added to it was a focus on the individual in the smaller context of life in the community, along with a strong sense of the role of ethnicity and religion . . . I was [now] deeply interested in the historical problems that focus on the immediate human context—the community and community change, religion and religious values, migration and mobility, education and the family." Hays's emphasis on ethnic and religious issues varied with circumstances and formed part of a much larger scheme.¹⁹

¹⁷ "Benson wrote *The Concept*," Michael Zuckerman said recently, "to get out from under a theory, a materialist theory that didn't work, in either the vulgar American version that oppressed him or the sophisticated Marxist version that obsessed him . . . [H]e never dreamed of discarding materialism. He simply tried to transcend its totalizing dominion, to fuse Marx with Merton, Tocqueville, and whatever else intelligence suggested to get a better handle on American life and its amelioration." Comment on "The Invention of the 'Ethnocultural Interpretation,'" American Historical Association Annual Meeting, December 29, 1992, Washington, D.C.

¹⁸ Samuel P. Hays, *Political Science Quarterly*, 70 (1965): 373–94. This essay had been anticipated by "History as Human Behavior," *Iowa Journal of History*, 58 (July 1960): 193–206. The latter devoted but two pages to cultural issues and voting patterns, 196–97.

¹⁹ Samuel P. Hays, *American Political History as Social Analysis* (Knoxville, Tenn., 1980), 11–13. See Hays's well-known essay, "Political Parties and the Community-Society Continuum," in *The American Party Systems: Stages of Political Development*, William Nisbet Chambers and Walter Dean Burnham,

Benson's 1957 essay "Research Problems" aimed at what now seems the astonishingly obvious matter of getting historians to stop studying elections one at a time as isolated events and to put them in perspective by looking at time series. Explaining elections was no simple matter; Benson insisted that

no interpretation of an election outcome can begin to be verified until the description of what happened is translated into who (voting groups) caused it to happen. The term "who" refers to any characteristic distinguishing certain voters from other voters . . . , [which] can range from full socio-economic descriptions to the extremely specialized case of voters who listened to a particular campaign address Broadly speaking, distinguishing features can be of two types: *group characteristics* of particular voters (religion, economic class, ethnic origins, urban-rural, socio-economic status, etc.); or *operative conditions* at a certain place at a certain time (economic depression, heightened sectional conflicts, intense religious or ethnic antagonisms, etc.).²⁰

Allan G. Bogue recently called attention to this essay's unprecedented discussion of research design: "[Benson] was throwing open the gate to the beckoning prairies of behavioralism as had no previous specialist in American history."²¹

Multicausality also pervaded Benson's collection of essays, *Turner and Beard* (1960). Ethnocultural politics was barely discussed, most of the book consisting of an attempt to distinguish between "economic determinism," which Benson rejected, and "economic interpretation," which he accepted, in the writing of the two Progressive masters.²² In the last third of the book, Benson considered Charles Beard's *Economic Interpretation of the Constitution*, as well as Beard's critics, and concluded that while the "economic determinist" parts of Beard's main hypothesis "will 'not work,' . . . [if] restated, some of his claims can be reincorporated into a *social* interpretation of the Constitution that may 'work.'" Benson then hypothesized that the struggle in the 1780s pitted elite partisans of the Agrarian Society against those of the Commercial Society. "The terms do not denote classes, socioeconomic groups, or occupations—they denote ways of thinking." Many farmers, for example, were commercial minded. This ideological conflict "in large part . . . stemmed from the 'modes and processes of gaining a livelihood' [Beard's

eds. (New York, 1967), 152–81, esp. 158–60. Ironically, the article perhaps most cited and reprinted from this part of his career is the urban politics classic "The Politics of Reform in Municipal Government in the Progressive Era," *Pacific Northwest Quarterly*, 55 (October 1964): 157–69, which is a view of urban Progressive reform as upper class and elite-driven; the New Left Students for a Democratic Society deemed it enlightening enough to reprint and distribute later in the 1960s. Hays's overall agenda differed from Benson's in important ways not discussed here, and their substantive work concentrated on different periods. Hays's focus on urban-industrial society in the late nineteenth and twentieth centuries also accounts for his receiving less attention here.

²⁰ Lee Benson, "Research Problems in American Political Historiography," in *Common Frontiers of the Social Sciences*, Mirra Komarovsky, ed. (Glencoe, Ill., 1957), 120–21, italics in original. Since later critics so often accused new political historians of inattention to class differences within ethnic groups, it is important to note that Benson clearly recognized in 1957 that members of an ethnic group could vary in their party attachments according to class differences within the group, p. 173.

²¹ Bogue, "Quest for Numeracy," 93.

²² Lee Benson, *Turner and Beard: American Historical Writing Reconsidered* (Glencoe, Ill., 1960). One can find here an explicit case of Benson arguing that in New York State's ratification voting an urban-agrarian split overrode ethnic and religious group differences, pp. 195–97. The point of this section was to show that a systematic analysis of voting by state was needed to test adequately propositions regarding sources of support or opposition to federalism, see pp. 194–202.

phrase].” Benson’s revised hypothesis “claims that social environment and position in the American social structure mainly determined men’s ideologies.”²³

In discussing academics’ fears during the 1950s that any association with Communism might cost them their jobs, Peter Novick quoted Benson’s recollection that *Turner and Beard* was written from an “implicit Marxian standpoint,” but when he wrote it Benson had no tenured position and was “afraid that [he] would never get one if [he] made [his] ‘Marxian standpoint’ explicit.”²⁴ Be that as it may, are there any grounds for the 1989 assertion of Terrence J. McDonald regarding “Benson’s assumption that there were no fundamental economic structural differences in American society”?²⁵

Some critics may have believed that *Concept* was written from an *anti*-Marxian position. It did challenge head-on the Progressives’ economic interpretation of Jacksonian political parties. But was it written in the same spirit of multicausality as Benson’s 1957 essay quoted above? Particularly impressive in this regard was Benson’s chapter, “American Voting Behavior,” the last section of which considered “Three Categories of Voting Determinants” and closed with a paragraph titled “Combinations of Determinants.” “I do not regard monistic explanations of voting behavior as credible,” wrote Benson, “whether the ‘single factor’ is ascribed to all men or to specific individuals and groups. ‘No man,’ Coleridge observed, ‘does anything from a single motive.’ Thus the problem of interpreting voting behavior at a given time and place is always to decide which *combination of determinants* influenced voters identified by some attribute, or set of attributes.”²⁶

IT HAS BEEN A PECULIARITY OF THE CANON that most of its creators did not engage *The Concept of Jacksonian Democracy* directly but concentrated on its successors, and in doing so most inventors missed encounters with passages such as the one just quoted. The skirting of *Concept* perhaps arose from the circumstance of a group of new political histories appearing in a cluster from 1969 to 1972, including books by Michael Holt, Frederick Luebke, Paul Kleppner, Richard Jensen, William Shade, Samuel McSeveney, and me.²⁷ In constructing the “ethnocultural

²³ The hypothesis also claims that “social ideologies and opinions on the Constitution were mainly determined by the combined effects of three related, somewhat overlapping factors: 1) men’s roles within the existing economic structure which strongly influenced their roles within the social structure; 2) the degree of urbanization of the areas in which they lived; 3) the ties which linked their communities with seaboard cities and interior large towns.” Benson, *Turner and Beard*, 215, 218, 223.

²⁴ Novick, *That Noble Dream*, 332. Benson’s “Group Cohesion and Social and Ideological Conflict: A Critique of Some Marxian and Tocquevillian Theories,” *American Behavioral Scientist*, 16 (June 1973): 741–67, is a Marx-derived critique of economic determinism; see also “Marx’s General and Middle-Range Theories of Social Conflict,” Robert K. Merton, James S. Coleman, and Peter H. Rossi, eds., *Qualitative and Quantitative Social Research: Papers in Honor of Paul F. Lazarsfeld* (New York, 1979), 189–209.

²⁵ Terrence J. McDonald, “The Burdens of Urban History: The Theory of the State in Recent American Social History,” *Studies in American Political Development*, 3 (1989): 17–18.

²⁶ Benson, *Concept*, 286–87.

²⁷ Michael F. Holt, *Forging a Majority: The Formation of the Republican Party in Pittsburgh, 1848–1860* (New Haven, Conn., 1969); Frederick C. Luebke, *Immigrants and Politics: The Germans of Nebraska, 1880–1900* (Lincoln, Neb., 1969); Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900*, 2d edn. (New York, 1970); Richard J. Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago, 1971); William G. Shade, *Banks or No Banks: The Money*

model," the inventors proceeded to lump together diverse books, skipping about from one to the other, undermining all with the most vulnerable points of each. At its best, this strategy illuminated significant differences, as well as some common strengths and flaws. Flaws there were, certainly, in these pioneering efforts of the new political history. The critics' method at its worst, however, concocted artificial differences and provided a license for critics to hold every "ethnoculturalist"—as defined by the critic—accountable for every statement made by every other "ethnoculturalist."

James E. Wright's 1973 essay, "The Ethnocultural Model of Voting: A Behavioral and Historical Critique," exhibited several features of the pattern just described. After noting that Benson and Hays "have played key roles in the growth of the ethnocultural model," Wright ignored them and focused on two books by Kleppner and Jensen dealing with the late nineteenth century—his own period of research. Wright offered six criticisms, the first of which J. Morgan Kousser reinforced three years later in formal statistical terms. Wright, in nonstatistical but sociological language that gave his prose a scientific aura, asserted that ethnoculturalists relied wrongly on "homogeneous units"—townships, for example, dominated by one group such as Irish Catholics or German Lutherans—to make inferences about individual behavior. This method could not account for the behavior of those same groups living elsewhere in smaller numbers or scattered through the population. Through the years, critics echoed commonsense variations of Wright's refrain or pointed to Kousser's statistical explanation of why this technique *could* lead to biased estimates.²⁸

Was it true that the new political historians did not understand that population mix and degree of acculturation might result in different behavior? In a discussion of a strongly Democratic, prosperous Dutch or "Yorker" town, Benson reasoned that "because Clarkstown constituted such a socially homogeneous community, it functioned as such a politically homogeneous one." He went on at length about the connections between interaction and communication, and identity and cohesiveness,²⁹ clearly indicating his awareness of the variations in behavior caused by "cultural mix" and relative acculturation. An explicit discussion of the problem of homogeneous units already existed in my own 1969 essay in the *Historical Methods Newsletter*, but it was seldom cited by the inventors.³⁰

Question in Western Politics, 1832–1865 (Detroit, Mich., 1972); Samuel T. McSeveney, *The Politics of Depression: Political Behavior in the Northeast, 1893–1896* (New York, 1972); and Formisano, *Birth of Mass Political Parties*.

For example, J. Morgan Kousser, "The 'New Political History': A Methodological Critique," *Reviews in American History*, 4 (March 1976): 1–13, dealt with my book, Kleppner's, and (to a lesser extent) Sheldon Hackney, *Populism to Progressivism in Alabama* (Princeton, N.J., 1969); Richard B. Latner and Peter Levine, "Perspectives on Antebellum Pietistic Politics," *Reviews in American History*, 4 (March 1976): 15–24, dealt with myself, Holt, and Shade.

²⁸ James E. Wright, "The Ethnocultural Model of Voting: A Behavioral and Historical Critique," *American Behavioral Scientist*, 16 (June 1973): 659–60; Kousser, "New Political History," 2–5; and see note 10 above.

²⁹ Benson, *Concept*, 296. See also Formisano, *Birth of Mass Political Parties*, 191–94, 299, for a discussion of variables affecting acculturation.

³⁰ Ronald P. Formisano, "Analyzing American Voting, 1830–1860: Methods," *Historical Methods Newsletter*, 2 (March 1969): 1–12. "These methods provide almost no way, however, for making reasonable inferences about individual members of social groups who are dispersed throughout the

The critics also glided over the fact that the self-conscious, systematic use of data from homogeneous local units was a significant advance over previous practices. In the early and mid-nineteenth century, many ethnic and religious groups settled in clusters on the American frontier, producing many culturally and socioeconomically homogeneous or majoritarian towns. Given the difficulties of assembling systematic demographic data to test even the simplest of hypotheses regarding nineteenth-century politics, surely Wright and Kousser were not suggesting that information about such places be discarded? Kousser was correct that the statistics employed by the first new political historians were not always well suited to the tasks at hand, but problems associated with statistical techniques and the quality of demographic data available for the nineteenth century have persisted.³¹ Kousser's emphasis on levels of significance was technically correct but abstracted from explanations that relied primarily on immersion in primary sources and systematic appraisal of quantitative data. Kousser lampooned as "gestalt" correlation what historians would learn to call, after Clifford Geertz, "thick description." Statistically meaningless, the "homogeneous units" did not lie about themselves, and there were plenty of them. As Yogi Berra once said, you can observe a lot just by watching.

Wright's other criticisms of Kleppner and Jensen will not be discussed in detail here. They dealt mostly with alleged problems in establishing the connection between religion ("doctrinal predilections") and political behavior. It needs to be pointed out, however, that Wright frequently couched his counter-propositions in vague and qualified terms, such as "must have been," "not conclusive of anything," and "may have been";³² he used occupation as a surrogate for class without mentioning the hazards of doing this for antebellum America;³³ he faulted "ethnoculturalists" for not controlling for class, then later in the essay said they had not controlled for status, without indicating how the two were conceptually different or how the practical problem of discriminating between class and status

population and presumably emotionally apart from the group, lacking ... reinforcement of perceptions and behavior patterns," p. 2.

A recent, prize-winning study of the New Deal relied on data from homogeneous census tracts (in a more systematic fashion than the early new political histories): Gerald H. Gamm, *The Making of New Deal Democrats: Voting Behavior and Realignment in Boston, 1920-1940* (Chicago, 1989). See pp. 27-28, 34-35, regarding method.

³¹ An irony has emerged from the debates over ecological regression, initially touted as a talisman for the allegedly misleading use of homogeneous and banner units (the latter being a party's top-ranking township or county). Regression makes the same assumption charged against the early new political historians, namely that members of a group behave the same way *no matter where they live*. For instance, it assumes that Irish Catholics who constituted 5 percent of a town's electorate will behave the same way, or vary randomly, as Irish Catholics who make up 90 percent of a town. Phyllis F. Field, "Nineteenth-Century American Voting Studies: A New Generation," *Historical Methods*, 22 (Fall 1989): 165; William G. Shade, "'New Political History': Some Statistical Questions Raised," *Social Science History*, 5 (Spring 1981): 177. For the pros and cons, see esp. William H. Flanigan and Nancy H. Zingale, "Alchemist's Gold: Inferring Individual Relationships from Aggregate Data," *Social Science History*, 9 (Winter 1985): 71-92, with commentary by J. Morgan Kousser, Robert R. Dykstra, and rejoinder by Flanigan and Zingale. For a recent unfavorable assessment of regression's "critical assumption," see James L. Huston, "Weighting, Confidence Intervals, and Ecological Regression," *Journal of Interdisciplinary History*, 20 (Spring 1991): 631-54.

³² Wright, "Ethnocultural Model of Voting," 662, 663, 665.

³³ Wright, "Ethnocultural Model of Voting," 663-64. A great many political and social studies deal with this issue, but so did Benson, *Concept*, 157-59.

might be handled;³⁴ and, finally and inexplicably, Wright charged new political historians with ignoring anti-Catholicism.³⁵ But Benson had made anti-Catholicism one of the central themes of his chapter "Religious Groups and Political Parties," while Kleppner, Jensen, and I had each discussed it extensively.³⁶

IN 1974 APPEARED WHAT WOULD BECOME the most influential and useful essay in the invention of the ethnocultural interpretation. Richard L. McCormick's "Ethno-Cultural Interpretations of Nineteenth-Century American Voting Behavior" analyzed carefully the books of Benson, Holt, Jensen, Kleppner, Luebke, McSeveney, and myself and began his critique, unusual for the inventors, with several paragraphs on Benson and Hays. McCormick dissected differences among "ethnoculturalists" for the purpose of showing that they relied on three different theories regarding "the translation of cultural differences into political ones." The three were, first, that "negative reference group feelings are the determinants of party affiliation"; second, that "ethno-cultural groups often seek political means of extending the dominion of their own cultural practices, or of protecting those practices from attack" (cultural defense or aggression); and, third, that "political affiliations reflect differences in religious beliefs and world views."³⁷ McCormick objected that these represent "different theories about the kinds of human motives which are the source of political activity." But he simply asserted and did not demonstrate that the "theories are strikingly different," a point hardly self-evident except to those choosing to believe it. Indeed, at one point, McCormick admitted that religious belief, lifestyle, and negative reference "impulses" *might be* "theoretically compatible" but then just repeated his objection without explanation.³⁸ McCormick also never explained what he meant by "theory," and he seemed to be talking about different ways of explaining political behavior that, in contrast to his claims, could overlap or reinforce one another. Negative reference group behavior and cultural defense, for example, are often the same thing.

One example of McCormick's approach in this regard must suffice here. He particularly faulted Benson's account of a New York county in which reference group perceptions, tradition, and religious values all combined to produce polarized Democratic voting. But instead of praising Benson's analysis because of the sturdiness of an explanation based on overlapping categories, McCormick

³⁴ Wright, "Ethnocultural Model of Voting," 662–64, 667–69.

³⁵ "Seymour Lipset . . . observed several years ago that four variables influenced 'the basic political differentiation among American religious bodies.' These were (1) social status, (2) economic class, (3) anti-Catholicism 'in both its religious and ethnic form,' and (4) the 'level of concern with 'public morality.'" The ethnoculturalists generally have focused on the last variable to the exclusion of the other three." Wright, "Ethnocultural Model of Voting," 669.

³⁶ Benson, *Concept*, 186–207; Kleppner, *Cross of Culture*, 40, 76–77, 103, 251–67; see the index entries for anti-Catholicism in Formisano, *Birth of Mass Political Parties*, 349; and Jensen, *Winning of the Midwest*, 339.

³⁷ Richard L. McCormick, "Ethno-Cultural Interpretations of Nineteenth-Century American Voting Behavior," *Political Science Quarterly*, 89 (June 1974): 358–59. In note 2, McCormick listed six previous evaluations of "ethnocultural history," but only one of those essays took "ethnocultural" as a theme, while the rest concentrated on the social science aspects of the new political history, p. 352.

³⁸ McCormick, "Ethno-Cultural Interpretations," 367.

called Benson's account "too good and airtight" because, by selecting "for close observation a county where so many ethno-cultural factors worked together, Benson has obscured our perception of any of them. Benson's technique . . . undercuts his own capacity to account for the diverse political affiliations of native American voters throughout the state." This criticism, too, was asserted and not shown, and it ignored Benson's consideration of the county in question as part of a larger discussion that aimed to do precisely what McCormick said Benson was not doing: in Benson's words, "to illustrate concretely the general conclusion that 'localistic' factors significantly influenced native Protestant voting patterns from 1826 to 1844." McCormick missed Benson's inclusion of non-ethnocultural factors and the point that Benson had contrasted polarized with unpolarized localities, leading Benson to an emphasis on "a multiplicity of 'localistic' factors" to explain the unpolarized,³⁹ in contrast to McCormick's claim.

One final point about this part of McCormick's essay dealing with what I have termed "ways of explaining": at the outset, McCormick referred to three "theories" of voting; he then called them "three hypotheses" and, among other terms, "three mechanisms."⁴⁰ In an essay demanding of others exacting standards of precision in the use of concepts, McCormick should have untangled and defined these very different things.

The principal impact of McCormick's essay, however, came from his argument in the last six pages that "ethnoculturalists" had not faced squarely the question "of what, if anything, mobilizing voters has to do with making policies." Only a small proportion of public policies in the nineteenth century were "culturally oriented," McCormick contended, and most government action had to do with economic policies. Thus the ethnoculturalists had virtually succeeded in detaching public policy from voting, yet they were unwilling to accept that separation and, worse, ignored the "origin of economic policies . . . Private motives of elites, as well as long-range patterns of social and economic development, are ignored as sources of economic policy."⁴¹

In 1979, Paul Kleppner replied to this criticism by pointing out that the voting studies did not speak to policy directly because they were not designed to do so. But McCormick's point about voting policy has had an enduring influence. Although unfounded as criticism, the question generated positive results in addition to confusion. It raised political historians' awareness regarding policy and helped breathe new life into the field.⁴² However, the early new political history did address linkages between policy and voting, although not in the narrow way that McCormick preferred to define the connection. Further, ignored in the subsequent discussion has been the fact that McCormick's question—What

³⁹ McCormick, "Ethno-Cultural Interpretations," 359–61; Benson, *Concept*, 291–93. In his next paragraph, McCormick wrote that Benson (and Hays) ignored instances in which voters were cross-pressured, when in fact Benson was the first historian to discuss systematically the concept of cross-pressures in voting behavior. See *Concept*, pp. 163–64.

⁴⁰ McCormick, "Ethno-Cultural Interpretations," 358, 371; they were also "ways of specifying," 359, "explanation," 361, and "kinds of links," 365.

⁴¹ McCormick, "Ethno-Cultural Interpretations," 371–77, quotations on 375.

⁴² Paul Kleppner, *The Third Electoral System 1853–1892: Parties, Voters, and Political Cultures* (Chapel Hill, N.C., 1979), 371. Kleppner replied to McCormick at length, pp. 371–82. For a typical repetition of McCormick's criticism, see Feller, "Politics and Society," 140, 144.

did mobilizing voters have to do with making policy?—did not derive from pure logic but rather from a set of premises about how policy is formulated and the role of elections in the process, that is, from a set of premises or beliefs about how politics works.⁴³ Of course, scholars in history and political science routinely disagree about how things work.

If a more responsible historiographical ethic prevailed in the discipline, Benson's first book, *Merchants, Farmers, and Railroads* (1955), long ago would have entered the discussion. It explained the making of a public policy, railroad regulation, by the actions of interest groups on public opinion and lawmakers. The first significant modification of laissez-faire in New York, he argued, came out of "conflicts between different sets of entrepreneurs adjusting to the new economic environment created by the development of a vast, intricate railroad network." The book drew careful distinctions within lobbying groups, and, except for passing comments, such as one about the Greenback Party's electoral strength rising and falling with economic conditions, Benson ignored popular voting because he found that public opinion had little impact on policy.⁴⁴

Given the norms of criticism, one can understand why McCormick ignored *Merchants, Farmers, and Railroads*. But, given his focus on books that appeared between 1969 and 1972, why did he omit discussing William G. Shade's 1972 book, *Banks or No Banks: The Money Issue in Western Politics, 1832–1865*, originally a dissertation influenced by Benson? It linked the formation of economic policy to the cultural coalitions making up the parties and also indicated that party shifts in banking policy responded to panics, depressions, railroad building, and other economic changes.⁴⁵

Because he operated from different—and limiting—premises, McCormick also did not discuss Benson's connections between policy and voting in *Concept*. In Chapter 5, "Positive versus Negative Liberalism," Benson laid out Whig and Democratic differences in relation to attitudes to government activity in promoting both economic and moral development. To illustrate, he discussed banking legislation from the 1790s to the 1830s, with attention to the interest groups (or "private motives of elites"), reformers, and political factions shaping policy.⁴⁶ Benson's chapter on religious groups also showed the ties between group dispositions and Whig moral meddling and the Democratic laissez-faire appeal—clear policy-voting linkage. But in "Interpreting New York Voting," he considered together cultural, economic, geographic, and developmental variables,

⁴³ Michael F. Holt recently discussed some of those premises explicitly, *Political Parties and American Political Development: From the Age of Jackson to the Age of Lincoln* (Baton Rouge, La., 1992), "Introduction," 23–28. See also Holt's essay, "The Election of 1840, Voter Mobilization, and the Emergence of the Second Party System: A Reappraisal of Jacksonian Voting Behavior," 151–91 (originally published 1985); and Ronald P. Formisano, "The New Political History and the Election of 1840," *Journal of Interdisciplinary History*, 23 (Spring 1993): 661–82.

⁴⁴ Lee Benson, *Merchants, Farmers, and Railroads: Railroad Regulation and New York Politics, 1850–1887* (Cambridge, Mass., 1955), vii–viii, 12–13, 81, 91, 94–114, 124–25, 128, 142, quotation on viii.

⁴⁵ See the favorable but not uncritical review of *Banks or No Banks* by George D. Green, *Business History Review*, 47 (Winter 1973), which closed, "Rather than engaging in superficial debate over the priority of economic versus ethno-religious factors in American politics, let us follow Professor Shade's own substantive leads to build a subtler, multi-variate synthesis encompassing both forces and their many interactions," p. 514.

⁴⁶ Benson, *Concept*, 86–109.

including "long-range patterns of social and economic development," and tied policy to group voting. It was not, however, a matter of showing how specific policies mobilized particular constituencies but rather how party programs and ideologies generally attracted or repelled various political subcultures. Democratic Rockland County, for example, had been settled by Dutch farmers and was a prosperous agricultural county with a political heritage of opposition to centralized authority and of resistance to change. Moreover, "the Erie Canal and similar 'internal improvements' tended in the short run to injure rather than benefit farming communities in eastern New York." The transportation revolution had passed them by, and Henry Clay's "American System" did not fire enthusiasm in Yorker Rockland. They recoiled from "positive government action designed to foster the 'general welfare'" and were instead "likely to respond to the party that preached the doctrines of the negative liberal state and state rights. Part of their tradition . . . was resistance to social order imposed by political government, as distinct from social order imposed by community mores and customs."⁴⁷ In rounding out his discussion of Rockland, Benson added Dutch hostility to Yankees and Negroes as reinforcing their attraction "to the party championing state rights and opposing positive state action."⁴⁸ Benson's many strands of connection from culture and condition to policy may have been all right, all wrong, or in between, but the notion that he posited no causal flow from voting to policy cannot be sustained.

In 1980, Ballard Campbell published a study of three midwestern legislatures in the 1880s and 1890s that is worthy of note here because it was designed to test directly the connection between policy and voting. McCormick had stressed that cultural issues made up only a small share of legislative business and economic policy the lion's share. But Campbell found that all "contested issues" constituted only a fraction of business, with commercial issues and community mores the two most contentious subjects. Community mores generated the most public attention, and economic policy took shape in a way not unlike Benson's description of railroad policy-making in New York: it "attracted lobbyists from entrepreneurial and occupational groups" who represented narrow economic interests and whose activities had limited public impact except in special cases. In contrast, cultural issues attracted broad pressure groups and voter attention.⁴⁹ Thus Campbell, too,

⁴⁷ Benson, *Concept*, 293–300, quotations on 297, 298, 299. In Rockland County, "appeals to 'economic interest' reinforced behavior patterns that long antedated Jackson's leadership . . . [M]ore significantly, Rockland attitudes and opinions on internal improvements must be seen as only one expression of a pervasive, deep-rooted cultural antipathy to state action of any kind, whether to improve transportation facilities (especially other areas' facilities), to 'regulate the consciences of men,' or to raise educational levels and intellectual capacities," p. 300.

⁴⁸ Benson, *Concept*, 303. His discussion of Yankee Chataqua County in western New York similarly wove together cultural and political heritage, long-term economic change, attitudes toward improvement, economic condition, geography, and negative reference behavior pointing toward a general policy outcome, pp. 304–17.

⁴⁹ Ballard C. Campbell, *Representative Democracy: Public Policy and Midwestern Legislatures in the Late Nineteenth Century* (Cambridge, Mass., 1980), 13–14, 19–21, 27, 57–78, 102, 103–04, 150–51, 156, quotation on 24. Robert H. Wiebe also accepted the disconnection between voting and specific economic policies in his important synthesis, *The Opening of American Society: From the Adoption of the Constitution to the Eve of Disunion* (New York, 1984), 349–50. In 1986, McCormick vaguely summarized Wiebe's point but still insisted on the centrality of economic issues to party contests. Richard L.

divorced specific economic policies from having consequences for electoral mobilization.

IN THE SAME YEAR that McCormick's essay appeared, 1974, Eric Foner in an influential article added another dimension to the critics' "invention." Even though he devoted only a few pages to the new political history, Foner succeeded in planting the notion that an "ethnocultural interpretation" of the Civil War existed. In addition, this "new mode of explanation" was "fundamentally ahistorical" and deterministic: "For the 'economic man' of the progressives, the new political history has substituted an equally one dimensional 'religious man.'" In delivering these salvos, Foner was hardly a disinterested critic. His essay indirectly defended his important book, *Free Soil, Free Labor, Free Men* (1969), major elements of which were contested by new political histories, but Foner chose not to join issue on substantive matters in favor of criticism of a larger target.⁵⁰ In 1985, Don E. Fehrenbacher considerably broadened Foner's argument with a vigorous assault on the "ethnocultural interpretation of the Civil War." Yet the books and essays on which Foner and Fehrenbacher concentrated their fire dealt primarily with the formation of the Republican Party in the North. Both gave the false impression that these works dealt with "Civil War causation" when in reality they dealt with the realignment of the 1850s in the North. When Fehrenbacher wrote that "Benson himself contributed several times to the discussion [of Civil War causation], but, with a few exceptions, the 'new political history' tended to deemphasize the sectional conflict," Fehrenbacher should have added *in the formation of the Republican Party during 1853–1856*.⁵¹ The fact is that no new political historian has written a full account of "the coming of the Civil War." Michael F. Holt's book *The Political Crisis of the 1850s* (1978) is an excellent brief account of northern politics but contains only one chapter on southern secession. (Fehrenbacher dismissed this still standard secondary source in one sentence.) Before 1978, Benson had given sustained attention to Civil War causation in general but only in papers and essays. Those essays did not discuss voting in the North because they were concerned with other matters. Fehrenbacher devoted a scornful paragraph to one of those essays collected in Benson's *Toward the Scientific Study of History* (1972) but mentioned no others. Foner referred to none.⁵²

McCormick, *The Party Period and Public Policy: American Politics from the Age of Jackson to the Progressive Era* (New York, 1986), pp. 131, 138.

For a pathbreaking analysis of ethnocultural voting and public policy, see Kathleen Neils Conzen, "German-Americans and Ethnic Political Culture: Stearns County, Minnesota, 1855–1915," John F. Kennedy-Institut für Nordamerikastudien, Working Paper No. 16, 1989. See also Peter Wallenstein, *From Slave South to New South: Public Policy in Nineteenth-Century Georgia* (Chapel Hill, N.C., 1987), 17, 21, 50–51, 95–96.

⁵⁰ Eric Foner, "The Causes of the American Civil War: Recent Interpretations and New Directions," *Civil War History*, 20 (September 1974): 199–200. For specific problems with Foner's interpretation of northern politics in the 1850s, see Ronald P. Formisano, "Communications," *Civil War History*, 21 (June 1975): 185–89.

⁵¹ Don E. Fehrenbacher, "The New Political History and the Coming of the Civil War," *Pacific Historical Review*, 54 (May 1985): 117–42, quotation on 125.

⁵² Fehrenbacher, "New Political History," 127–28. Lee Benson, *Toward the Scientific Study of History*:

Fehrenbacher did allude to his differences with Holt and me regarding the mid-1850s. "The struggle over the Kansas-Nebraska Act, they maintained, was of less importance in bringing about the disruption of the second party system than ethnic and religious conflicts at the local level over such issues as prohibition, sabbatarianism, and Know-Nothingism." Fehrenbacher accused us of "oversimplification" and promptly nailed down his point with a reference to a short book by Richard Jensen and a citation to that book dealing with the years 1888–1893.⁵³ He then asked a series of questions about the 1850s. Reading him, one would not guess that Holt and I actually had addressed these issues but that Fehrenbacher disagreed with *how* we addressed them and, most of all, with our emphasis on antisouthern and antislaveholder sentiment in contrast to his preferred emphasis on moral antislavery. In a footnote, he referred to a forthcoming book by William E. Gienapp that promised "to shed much new light on the emergence and triumph of the Republican party." That same year, however, Gienapp published an article explicitly challenging accounts that minimized the role of nativism in the formation of the Republican Party, and the promised book has since reasserted in overwhelming detail the importance of cultural issues in the period up to 1856. Like Holt, Silbey, and others, Gienapp understood the increasing impact of sectionalism through the 1850s but not in the way insisted on by Fehrenbacher.⁵⁴ Gienapp's and other recent writing in this field suggests that the differences between new political histories and Foner and Fehrenbacher may not be as great as the latter insisted.

Among Fehrenbacher's several criticisms of the "ethnocultural interpretation," perhaps the most serious were his repetition of the charge of religious determinism and the claim that ethnoculturalism in its "most reductive version" divided all voters into one or another religious category, such as "evangelicals" or "non-evangelicals" (a reference to my work). These categories had been inspired by Benson's chapter on religion and his discussion of Whig-leaning "puritans" and Democratic-tending "nonpuritans." Fehrenbacher followed the inventors in ignoring two points: first, Benson was establishing the political salience of groups that felt intensely for or against the moral regulation of society; and, second, even though Protestant-Catholic controversy was on the rise, for non-Catholic voters

Selected Essays of Lee Benson (Philadelphia, 1972). Silbey had discussed Civil War causation in two essays, one of which Fehrenbacher reviewed at length: "The Surge of Republican Power: Partisan Antipathy, American Social Conflict, and the Coming of the Civil War," in Stephen E. Maizlish and John J. Kushma, eds., *Essays on American Antebellum Politics, 1840–1860* (College Station, Tex., 1982), 198–229. Among other works not discussed by Fehrenbacher, see Robert Kelley, *The Cultural Pattern in American Politics: The First Century* (New York, 1979).

⁵³ Fehrenbacher, "New Political History," 133, 134, n. 40, 134.

⁵⁴ Fehrenbacher, "New Political History," 133–34, 136–37. Fehrenbacher also included an approving citation of Dale Baum, *The Civil War Party System: The Case of Massachusetts, 1848–1876* (Chapel Hill, N.C., 1984), which supported the Foner-Fehrenbacher interpretation. William E. Gienapp, "Nativism and the Creation of a Republican Majority in the North before the Civil War," *Journal of American History*, 72 (1985): 529–59, explicitly challenged Baum's interpretation as advanced initially in "Know Nothingism and the Republican Majority in Massachusetts: The Political Realignment of the 1850s," *Journal of American History*, 64 (1978): 959–86; and the book is Gienapp, *The Origins of the Republican Party, 1852–1856* (New York, 1987). It is probably better to wait until an egg has hatched before describing the chick—old Kansas saying. See also Michael F. Holt, "The New Political History and the Civil War Era," *Reviews in American History*, 13 (1985): 60–69.

"sectarian religious affiliations would not have provided a significant political differentiation."⁵⁵ Some misunderstanding might have been allayed if Benson had made it clear that his generalizations held best for puritans and antipuritans, with nonpuritans denoting a residual category; similarly, I should have used nonevangelical as a neutral group to indicate that not all voters were intensely evangelical or antievangelical.⁵⁶

The matter of religion cannot be left without challenge to the oft-repeated charge that ethnoculturalists ran statistical correlations between "church doctrines and political behavior."⁵⁷ Whatever other historians may have said about this subject, Benson has just been quoted above to the effect that Protestant denomination did not provide a guide to political difference. In similar fashion, in my own work, I had remarked, "'Religion' here clearly does not mean fine-spun theology, or specific doctrines and institutions. Rather, it implies common group dispositions." This did not prevent Richard Latner and Peter Levine from expanding on McCormick's charge that I assumed that "rigid denominational creeds are conveyed to members of the group."⁵⁸ Instead, I stressed that evangelicalism cut across denominations, and thus "evangelical groups contained many nonevangelicals, and vice versa." Because religious data were so obviously incomplete, I, like Benson, engaged in some systematic eyeballing, and not statistical handling, of evidence, some of which was in crude numerical form.⁵⁹

⁵⁵ Fehrenbacher, "New Political History," 123; Benson, *Concept*, 186–207, quotation on 186. The Whig-puritan and Democrats-nonpuritan "inference refers to central tendencies, not to perfect or near perfect relationships. To draw that inference in no way denies that attitudes and opinions related to other issues, interests, loyalties, and antagonisms exerted cross-pressures on voters. Nor is it denied that some men who might be classified as puritans opposed the Whigs on the ground that the state had neither the right nor the ability to legislate morality," *Concept*, p. 207.

⁵⁶ In 1979, Kleppner discussed at length a non-dichotomous scale of religious groups in "more or less" fashion (rather than "either-or"). *Third Electoral System*, pp. 185–97; see also Philip R. VanderMeer, "Religion, Society, and Politics: A Classification of American Religious Groups," *Social Science History*, 5 (Winter 1981): 3–24. John F. Reynolds, "Piety and Politics: Evangelicalism in the Michigan Legislature, 1837–1860," *Michigan History* (Winter 1972): 323–51, showed that I underestimated (Formisano, *Birth of Mass Political Parties*, 102–64) the degree of evangelical voting by Democrats in the legislature. Reynolds did not, however, contradict the central tendencies, and he showed the influence of locality on legislators' votes.

⁵⁷ "The ethnocultural interpretation has taken root because quantitative historians have run regressions between party affiliation and attendance estimates of each denomination as provided by the census. As many critics have pointed out, statistical relationships based on church attendance and party affiliation yield weak speculations as to the relationship between church doctrines and political behavior." James L. Huston, "Missing Links? Evangelicalism and Antislavery," *Reviews in American History*, 19 (December 1991): 496. Benson ran no regressions and used no census religious data.

⁵⁸ Benson, *Concept*, 192; Formisano, *Birth of Mass Political Parties*, 137–38. Compare my statements on 137–39, 141, 152, and 160–64 with Richard B. Latner and Peter Levine, "Perspectives on Antebellum Pietistic Politics," *Reviews in American History*, 4 (March 1976). Latner and Levine charged that the "new political history . . . attributes certain values, such as benevolence, moral reform, and piety, to *selected denominations* . . . [italics mine] Formisano's . . . interpretive model assumes that rigid denominational creeds are conveyed to members of the group who, in turn, translate them into political terms. As Richard L. McCormick astutely points out, ethnocultural historians thereby claim that religious leaders transmit doctrines to their flock while political leaders cannot do the same," pp. 19, 20.

⁵⁹ Formisano, *Birth of Mass Political Parties*, 137–38. Inventors have persistently ignored the following: "The number of seats did not measure membership. Many if not most communicating members of Protestant churches were women and children. Towns listing no churches in the census often had persons attending services in nearby towns. A 'Methodist' church may, in a poor rural area, have been attended by Baptists, or others. Towns remaining thinly settled or lacking a village center may have been peopled with religious men. The census manuscript of population lists many

BY THE TIME FEHRENBACHER PUBLISHED HIS ESSAY, the essentials of the invention of "ethnocultural interpretation" were in place, and he basically extended it. Similarly, Sean Wilentz, in his article "On Class and Politics in Jacksonian America" (1982), presented a modified version of the invention as a prelude to his own imaginative proposals that Jacksonian politics be regrounded on "republicanism" and on a new definition of class derived from E. P. Thompson and Herbert Gutman. Wilentz began his critique of ethnoculturalists (or "revisionists") with a frontal assault on Benson, summarizing Benson's challenge to Progressive history. This included Wilentz's assertion that Benson had depicted party ideologies as insignificant, "variants of liberalism, and little more." Then, Richard P. McCormick's "exceptional study" of party formation in the states "criticized Benson on some matters, [and] eschewed multivariate analysis [implying Benson had used it] . . . but appeared to clinch the argument that the 'second party system' aimed to eliminate ideology and touchy issues from politics, not to agitate them." Wilentz, however, overlooked a vital difference between McCormick and Benson. In contrast to Benson, who took seriously the differing programs of Democrats and Whigs, McCormick expressed his interest specifically in "questions other than those of ideology and constituencies." It is ironic that Wilentz misconstrued Benson, since he was perhaps the first to take seriously Whig ideology, opening the path to the work of such historians as Daniel Howe and Major Wilson.⁶⁰ Thus Wilentz began by misrepresenting Benson on a fundamental point.

From there, he proceeded to describe how "the 'new,' 'scientific,' political histories quickly hardened into an orthodoxy, a veritable ethnocultural school of American politics united by their belief that American political (and by inference, social) divisions are explicable in terms of ethnicity and religion and not of class." This school "so thoroughly marginalized class and economic change that class relations began to disappear from explanations of Jacksonian political history altogether."⁶¹

Wilentz then related how the "orthodoxy" quickly unraveled. First came "demurrers" and "second thoughts"; worse, political history became irrelevant to most social historians; finally, in the mid-1970s, came the barrage from critics who exposed the "serious flaws" of the voting studies and showed definitively that the revisionists "construed politics far too narrowly, and thereby distorted social relations, social consciousness, and the exercise of political power." Wilentz's litany, however, contained basic problems of chronology and substance. Close

'clergymen' in towns with no churches, and county histories often tell of 'societies,' classes, or organizations lacking buildings. Thus, a religious index based on seats is several times removed from the adult male electorate of voters. Merely quantifying the available data for religion gives it a concrete substance it does not possess." Formisano, *Birth of Mass Political Parties*, 139.

⁶⁰ Sean Wilentz, "On Class and Politics in Jacksonian America, *Reviews in American History*, 10 (December 1982): 45-63, quotations on 46-47. McCormick followed the political scientist Maurice Duverger in "understanding that American parties are above all electoral machines, engaged in nominating and electing candidates." Richard P. McCormick, *The Second American Party System: Party Formation in the Jacksonian Era* (Chapel Hill, N.C., 1966), 4. On the connections between Benson and Howe and others, see William G. Shade, "Politics and Parties in Jacksonian America," *Pennsylvania Magazine of History and Biography*, 110 (October 1986): 483-507.

⁶¹ Wilentz, "On Class and Politics," 47.

examination of his essay reveals that some historians were having second thoughts before they had first thoughts. Class was disappearing from Jacksonian historiography, Wilentz claimed, but several of his citations contradicted that assertion, as did his discussion, later in the essay, of Edward Pessen's valuable *Riches, Class, and Power before the Civil War* (1973), portions of which had already appeared.⁶²

Wilentz's quarrel with Benson centered on two points: that Benson's positing of a liberal political culture missed the opponents of liberal capitalism who expressed their class discontents through republicanism and, second, that the question of whether the lower classes voted for "their own party, to counter the opposing party of property," as the Progressives had framed it, was far too narrow. In the end, however, what Wilentz proposed as an alternative "general pattern" possessed a strangely Bensonian tone: "while both Whigs and Democrats were liberal parties led by different members of new and existing elites, the Whigs *tended* to draw their *popular* support chiefly from men who believed that they (and the Republic) benefited from the ongoing transformations of American market and class relations. The Democrats *tended* to appeal to those who did not." Ironically, the key book that Wilentz cited as authority for his statement that the quarter-century 1825–1850 "saw a rapid acceleration of American capitalist development and class formation" was George Rogers Taylor's *Transportation Revolution, 1815–1860* (1950), the book that Benson had singled out at the beginning of *Concept*. Because of economic growth and a rising level of aspiration, Benson believed, "a strong causal relationship existed between what George R. Taylor called the Transportation Revolution and the egalitarian movements that lead [Benson] to call the years from 1825 to 1850 the Age of Egalitarianism." As shown above, Benson had connected the economic changes of the transportation revolution to political reactions. Although Wilentz's description of economic change was not the same as Benson's, it was similar. Stripped from the partisan pattern, however, were religion, ethnicity, and race.⁶³

⁶² Wilentz's assertions and the relevant notes appear in "On Class and Politics," 47–48, 60. Of particular interest also are the citations to this statement: "In some cases, reassembly of the revisionists' voting data and use of more exact poll book records (when possible) yielded results at odds with the ethnocultural formula about politics and class," p. 48. Of the five sources given, one was an unpublished (student?) paper; one was Latner and Levine's *Reviews in American History* critique, which did not reassemble data; another was "Paul L. [sic] Bourke and Donald A. DeBats, 'Identifiable Voting in Nineteenth-Century America: Toward a Comparison of Britain and the United States Before the Secret Ballot,' *Perspectives in American History* 11 (1977–78): 257–88" [the first page is 259], which did not focus on questions of class and culture but rather stressed neighborhood and influentials in a Virginia county, pp. 277–86; and a fourth was a citation of the discussion in my 1971 Michigan book of two lists of voters kept by party operatives, which were incomplete and quite different from officially recorded "poll books," which did not exist in Michigan. Thus, in the course of pointing to the "serious flaws" of the voting studies, not only did Wilentz misidentify the Michigan voter lists as "poll books," but they had nothing to do with Jacksonian voting; one list was for Detroit in 1856 and the other for Lansing in 1858. In addition, I used them to argue that "[r]eligion, ethnicity, and economic role seem to have been the most salient influences on voting," along with urban-rural differences. Throughout, I had pointed out that various groups composing the Republican coalition had risen in prosperity and status since Michigan's frontier days. According to Wilentz, such analysis could not be found in "revisionists'" studies. Formisano, *Birth of Mass Political Parties*, 291–92, 294, 297–98 (the latter two are the pages Wilentz cited, along with 318–23).

⁶³ Wilentz, "On Class and Politics," 50–56, quotations on 50, 58, 57; Benson, *Concept*, 12–14. In contrast to Wilentz, Amy Bridges' more detailed analysis of voting in New York City better reflects the evolution of political history in dealing with limited data, *A City in the Republic: Antebellum New York and the Origins of Machine Politics* (New York, 1984). Bridges preferred to emphasize the class basis of

In his acclaimed book, *Chants Democratic* (1984), Wilentz asserted that Benson had not only missed the expression of class in politics but had also claimed that "ethnicity or consensus negated class differences." Wilentz turned Benson's thesis that class consciousness had not been expressed to a significant degree in the two-party system into the very different thesis that class differences had not existed in American society.⁶⁴ Yet Wilentz's own evidence for the presence of class conflict in mainstream politics was a single election: the 1829 New York City contest for state assembly in which a radical artisan Workingmen's Party won 31 percent of the vote; "an impressive debut," wrote Wilentz. The following year, however, the party split into three segments, with the "entrepreneurial" wing receiving 34.9 percent and two radical factions 10.7 and 0.6 percent of the vote. Then it disappeared; some debut. Wilentz's discussion of "rich," "poor," and "wealthier wards" and his table of uncorrelated data for fourteen wards were, by Kousser's standards, useless. Wilentz's fascinating book, while showing a rich variety of republican dissenters opposed to centrist politics and entrepreneurial republicanism, was innocent of any discussion of "the exercise of political power," for which lack he had chastised ethnoculturalists.⁶⁵

WHILE WILENTZ WAS FORCING CLASS CONFLICT BACK INTO POLITICS, most studies in the new labor, working-class, and urban history during the 1970s and 1980s were reaffirming, with a few exceptions, that socioeconomic and class divisions were not the primary determinants of voting. Although they found different amounts of class consciousness at workplaces or in public life in the communities examined, these studies agreed that class politics were at best weak or episodic. Wilentz, too, had seconded the new political history in rejecting the Progressive view of things. But most of the new working-class and urban histories went further and held that cultural divisions contributed powerfully to preventing class politics and that the major parties responded to and manipulated cultural, religious, and racial groups with the same result.⁶⁶

politics, but she paid considerable attention to cultural conflict. In a careful analysis (pp. 62–70), Bridges admitted that the evidence did not permit reliable estimates of exactly how working-class voters divided between the Whigs and Democrats, and she repeated Benson's observation that "the narrow margin between the parties shows that voting in New York City could not possibly have followed strict class lines" (Benson, *Concept*, 143.) Bridges' important account unfortunately included the claim that "Benson did not examine the voting behavior of New York City's electorate" and rather "inferred the dispositions of the city's ethnic groups from the behavior of similarly defined groups upstate" (p. 63). But Benson analyzed, albeit briefly, voting, economic, and ethnic data for the city's wards taken from the 1845 state census, and historical and contemporary descriptions. See Benson, *Concept*, 143–44, n. 31 on p. 144.

⁶⁴ Sean Wilentz, *Chants Democratic: New York City and the Rise of the American Working Class, 1788–1850* (New York, 1984), 7–9, quotation on 8.

⁶⁵ Wilentz, *Chants Democratic*, 190–210, 408, quotation on 198.

⁶⁶ Thus in a recent synthesis of nineteenth-century labor history, one finds the sensible conclusion that in Europe political and class lines paralleled one another, while in the United States "party lines crossed class boundaries. Party organizations cut deeply into the social structure and thwarted the politics of class." Bruce Laurie, *Artisans into Workers: Labor in Nineteenth-Century America* (New York, 1989), 79, see also 53–54, 56, 71–72. For a good general discussion of the fragmentation of the working class (even though it exaggerates its size and capacity), see Mike Davis, *Prisoners of the*

Similarly, an essay by Richard Oestreicher in 1988 synthesizing the new labor and new political histories, though dealing with a later period, largely supports this point, if for different reasons. Oestreicher constructively gave a non-reductive reading of the new political history and heroically tried to find common ground between two groups of historians who have too often ignored, from hostility or indifference, one another's work. Oestreicher carefully catalogued "sporadic class-oriented political challenges" from the 1870s through the 1930s and concluded that "at least diffuse forms of class sentiment were endemic among the working-class electorate." But, until the 1930s, this class sentiment usually "had no place to go politically," being pinned down by institutional and organizational barriers to its mobilization. "What was most distinctive about the political behavior of American workers in those years was the way in which the fit between their particular motivations and personal agendas and the structure of power in American society limited the translation of class sentiments into political consciousness."⁶⁷

In pointing to the convergence of labor and political history, Oestreicher was hardly declaring class irrelevant to politics. And he echoed Wilentz's discomfort with Benson's (and the new political history's) positing of a liberal tradition that erased (or limited) the potential for conflict. The surges of protest politics, he concluded, should arouse doubts "that a liberal tradition, or some underlying ethos in American culture, precluded the development of class consciousness in American society." Oestreicher instead stressed the agency of politicians who deflected the expression of class sentiment by cooption, promises, and symbols. "Politicians seem to have taken the existence of class sentiment more seriously than many political historians."⁶⁸ Nevertheless, Oestreicher ends with the same result as the political historians, who emphasized the internal fragmentation of the working class. Class politics may not have been "precluded," but it was normally suppressed or deflected.⁶⁹

American Dream: Politics and Economy in the History of the US Working Class (London, 1986), 14–15, 18–29. My sample of individual case studies includes Michael H. Frisch, *Town into City: Springfield, Massachusetts, and the Meaning of Community, 1840–1880* (Cambridge, Mass., 1972), 39, 127–28; Susan E. Hirsch, *Roots of the American Working Class: The Industrialization of Crafts in Newark, 1800–1860* (Philadelphia, 1978), xviii, 109, 110, 120, 124; although it deals with a later period, Daniel J. Walkowitz's excellent study, *Worker City, Company Town: Iron and Cotton-Worker Protest in Troy and Cohoes, New York, 1855–84* (Urbana, Ill., 1978), 255–56; Bruce Laurie, *Working People of Philadelphia, 1800–1850* (Philadelphia, 1980), 52, 66, 93, 110–11, 115, 202–03; Mary P. Ryan, *Cradle of the Middle Class: The Family in Oneida County, New York, 1790–1865* (New York, 1981), 130–31; Jonathan Prude, *The Coming of Industrial Order: Town and Factory Life in Rural Massachusetts, 1810–1860* (New York, 1983), 217–19, 254–55; and, especially, Steven J. Ross, *Workers on the Edge: Work, Leisure, and Politics in Industrializing Cincinnati, 1788–1890* (New York, 1985), 44–63, 181–92, n. 40 on p. 342.

⁶⁷ Richard Oestreicher, "Urban Working-Class Political Behavior and Theories of American Electoral Politics, 1870–1940," *Journal of American History*, 74 (March 1988): 1257–86, quotations on 1282, 1286.

⁶⁸ Oestreicher, "Urban Working-Class Political Behavior," 1282.

⁶⁹ Recent labor and immigrant historians also have refined considerably the understanding of ethnicity with which the early new political historians worked and its relationship to class and politics. Aware of the instrumental, dynamic quality of ethnicity, they have underscored the role of culture and other factors in fragmenting the working class. While differing in the relative emphasis accorded to culture and class, they have stressed the frequent blending of the two. See David A. Gerber, *The Making of an American Pluralism: Buffalo, New York, 1825–60* (Urbana, Ill., 1989); David M. Emmons, *The Butte Irish: Class and Ethnicity in an American Mining Town, 1875–1925* (Urbana, Ill., 1989); Gary

Few historians today would disagree with the proposition that, *relatively* speaking, the most intense class consciousness displayed throughout American history has been that informing the actions of political and economic elites. Elites also enjoyed access to a preponderance of legal and extra-legal force.⁷⁰ The new labor histories on balance indicate that the liberal republican tradition, as reflected in producer ideology, contributed to the ideological hegemony of those elites. Artisans' and workers' acceptance of producer ideologies, in addition to cultural and racial divisions, forestalled and inhibited class action in mainstream politics in their own interest. While class tensions and labor strife often erupted outside of party politics, the voting studies were guilty not of denying but of simply ignoring them because they were not frequently expressed directly in electoral politics.⁷¹ Nor did Benson and Hays deny the need to understand class structure or to determine who had power. Who gets what, why and how was a Benson mantra to his graduate students.

The new political history did show that specific economic policies were not routinely connected to the mobilization of voters, holding instead that the parties' general posture toward the use of government to promote economic development *and* to regulate social morality and mores did matter to the electorate. In nineteenth-century cities, as Robin L. Einhorn has shown recently in her innovative study of Chicago, the removal of economic policy from the democratic process was routine. A suggestion Einhorn makes—that “segmented governance” helps explain the blocking of working-class politics described by Oestreicher—fits with the findings of the new political history regarding voting and policy.⁷²

Gerstle, *Working-Class Americanism: The Politics of Labor in a Textile City, 1914–1960* (New York, 1989); Lizabeth Cohen, *Making a New Deal: Industrial Workers in Chicago, 1919–1939* (New York, 1990). See also Kathleen Neils Conzen, David A. Gerber, Ewa Morawska, George E. Pozetta, and Rudolph J. Vecoli, “The Invention of Ethnicity,” *Journal of American Ethnic History*, 12 (Fall 1992): 3–41.

⁷⁰ Economic and political elites at the local, regional, or national level were not always united and self-conscious about their goals, but on balance they were distinctively more conscious and cohesive in pursuit of their goals than artisans, workers, and laborers.

⁷¹ Two provocative essays by Michael Kazin reinforce the line of argument here, “A People Not a Class: Rethinking the Political Language of the Modern U.S. Labor Movement,” in Mike Davis and Michael Sprinker, eds., *Reshaping the US Left: Popular Struggles in the 1980s* (London, 1988), 257; and “Struggling with the Class Struggle: Marxism and the Search for a Synthesis of U.S. Labor History,” *Labor History*, 28 (Fall 1987): 497–514.

⁷² Robin L. Einhorn, *Property Rules: Political Economy in Chicago, 1833–1872* (Chicago, 1991), 7–103, esp. n. 41 on 19–20; also relevant, L. Ray Gunn, *The Decline of Authority: Public Economic Policy and Political Development in New York State, 1800–1860* (Ithaca, N.Y., 1988), originally a dissertation directed by Richard L. McCormick.

Two influential southern studies have formed the principal basis for an interpretation of antebellum politics emphasizing above all the transition to market capitalism. J. Mills Thornton III, *Politics and Power in a Slave Society: Alabama, 1800–1860* (Baton Rouge, La., 1977); and Harry L. Watson, *Jacksonian Politics and Community Conflict: The Emergence of the Second American Party System in Cumberland County, North Carolina* (Baton Rouge, 1981). Inventors of the “ethnocultural interpretation” have welcomed these studies but have not paused to note that in data and method they replicate the earliest of the new political histories. In extending the interpretation to the nation in *Liberty and Power: The Politics of Jacksonian America* (New York, 1990), Harry L. Watson tended to see cultural and economic divisions as reinforcing and “deeply intertwined,” p. 237, although sometimes he did not, p. 195. In contrast, Charles Sellers, *The Market Revolution: Jacksonian America, 1815–1846* (New York, 1991), ignored cultural factors or reduced them to an expression of class. Watson’s interpretation of North Carolina has been challenged by, among others, Thomas E. Jeffrey, *State Parties and National Politics: North Carolina, 1815–1861* (Athens, Ga., 1989).

THIS ESSAY HAS TRIED TO RECOVER what the new political history was mainly about, particularly in the case of Benson and Jacksonian politics. The purpose has not been to synthesize the current literature on antebellum politics. For that, readers may turn to Robert Kelley, Jean Baker, Joel Silbey, Michael Holt, William Gienapp, or to Richard L. McCormick's 1986 book of essays. A brief recent version can be found in Daniel Howe's fine synthesis, "The Evangelical Movement and Political Culture in the North during the Second Party System," which argues that "evangelical religion interacted with economic development [from 1830 to 1860] to polarize the population, creating the basis for two broad [partisan] alliances."⁷³

It needs to be added that when the inventors accused the new political historians of being ahistorical and determinist, they denied those historians their contribution to one of the most important developments in recent political-social history: sensitivity to context and local variation. Once we cut away the inventors' confusions, we realize, as Michael Zuckerman has, that the new political history joined social history not in some spurious "consensus history" but rather in the "disaggregation of the social structure." Benson and his followers, Zuckerman continued, insisted on "the importance of particular local contexts . . . 'few people ever lived in the age as a whole.' The age came to them 'filtered through mediating influences.'"⁷⁴

In the last two decades, awareness of community context has been heightened among historians of voting, parties, and political culture.⁷⁵ Sometimes, in certain meticulous case studies, context has threatened to overwhelm the historian's capacity to make any firm generalization at all.⁷⁶ But excursions into localities have brought a new understanding of neighborhood, kinship, and economic networks and of local influentials and patriarchs in shaping politics.⁷⁷ Paul F.

⁷³ Daniel Howe, *Journal of American History* (1991): 1212; Kelley, *Cultural Pattern in American Politics*; Jean H. Baker, *Affairs of Party: The Political Culture of Northern Democrats in the Mid-Nineteenth Century* (Ithaca, N.Y., 1983); Silbey, *American Political Nation*; Holt, *Political Parties and American Political Development*; Richard L. McCormick, *Party Period and Public Policy: American Politics from the Age of Jackson to the Progressive Era* (New York, 1986). The forthcoming book by Paul F. Bourke and Donald A. DeBats, *Washington County*, will be an important contribution to this literature.

⁷⁴ Michael Zuckerman, "Myth and Method: The Current Crisis in American Historical Writing," *History Teacher*, 17 (February 1984): 228, 230.

⁷⁵ Three examples of this trend, which is only one dimension of these complex works: Randolph A. Roth, *The Democratic Dilemma: Religion, Reform, and the Social Order in the Connecticut River Valley of Vermont, 1791-1850* (Cambridge, 1987); William G. Shade, "Society and Politics in Antebellum Virginia's Southside," *Journal of Southern History*, 43 (May 1987): 163-93; and John L. Brooke, *The Heart of the Commonwealth: Society and Political Culture in Worcester County, Massachusetts, 1713-1861* (Cambridge, 1989). In a recent phone conversation (November 29, 1992), Brooke told me that his work was conceived with Benson's *Concept* and the Benson-Hays social analysis of political culture in mind.

⁷⁶ Two well-crafted, conscientious examples of this statement are Paula Baker, "The Culture of Politics in the Late Nineteenth Century: Community and Political Behavior in Rural New York," *Journal of Social History*, 18 (Winter 1984): 167-93; and Paul Goodman, "The Social Basis of New England's Politics in Jacksonian America," *Journal of the Early Republic*, 6 (Spring 1986): 23-58.

⁷⁷ Goodman, "Social Basis of New England's Politics," 44-47; family and neighborhood networks were important in Hunterdon County, New Jersey, in the nineteenth century, Baker, *Affairs of Party*, 45-52; in Sugar Creek, Illinois, in the 1830s, "party identification was a family affair," reinforced by economic role and (for Whigs) propertied stability in the community, John Mack Faragher, *Sugar Creek: Life on the Illinois Prairie* (New Haven, Conn., 1986), 149-50, 160, 185, 190-96. Regarding neighborhoods and leaders in the South, see Robert C. Kenzer, *Kinship and Neighborhood in a Southern*

Bourke and Donald A. DeBats, especially, have emphasized the importance of spatial and small communal relationships in a series of articles over the past fifteen years reporting on their investigation of official poll book records of individual *viva voce* voting in several states. Indeed, they argue in their most recent oral presentations that these do not necessarily reflect cultural or religious groupings, although often they did. From the beginning of their work, however, Bourke and DeBats have stressed the historicity of what they have consistently referred to as "the traditional electorate."⁷⁸

The new political history has also encouraged a broad concern with political culture as a way of understanding belief systems, expressive symbols, habits of thought, and the things that people in the past took for granted. This approach complements the discovery of context and shares with it an appreciation of the past on its own terms, as recent works focusing on political culture have demonstrated.⁷⁹

In the three decades since the publication of *The Concept of Jacksonian Democracy*, many impulses have pushed and drawn historians along these paths. But an appreciation of Benson's legacy to these developments is long overdue. It might be said of Benson what Keith Thomas wrote recently about Lewis Namier: "By ignoring the views of previous historians and refusing to accept unverified assumptions, however longstanding, Namier demonstrated that there was no substitute for direct and laborious engagement with the original sources. In words that foreshadow much present-day historical thinking, he asserted that 'one has to steep oneself in the political life of a period before one can safely speak, or be sure of understanding, its language.'"⁸⁰

In pointing to the errors of the inventors of the ethnocultural interpretation, I do not hold that the new political histories were free of error. I am suggesting that historiographic criticism is a difficult art and that prevailing norms leave much to be desired. Gene Wise's plea in 1973 that historiography be taken seriously as a subfield has gone unheeded, as has his incisive analysis of the unspoken assumptions and shortcuts that we build into any generalizing about "schools" and the complexities inherent in what is done routinely and casually.⁸¹ In 1973, the *Journal of American History* listed twenty-two subfields in its guide to recent publications. By June 1993, that number had grown to forty-one, including such

Community: Orange County, North Carolina, 1849-1881 (Knoxville, Tenn., 1987), 6-51; Jeffrey, *State Parties and National Politics*, 146-47; and Christopher Morris, "Town and Country in the Old South: Warren County, Mississippi" (Ph.D. dissertation, University of Florida, 1991).

⁷⁸ See Paul F. Bourke and Donald A. DeBats, "Reconstructing the Traditional Electorate in the United States: The Pollbook Cases," paper presented at the Organization of American History Annual Meeting, 1991.

⁷⁹ See Baker, *Affairs of Party*, 12; and Ronald P. Formisano, *The Transformation of Political Culture: Massachusetts Parties, 1790s-1840s* (New York, 1983), 4, for definitions of political culture. Besides these, recent examples include Brooke, *Heart of the Commonwealth*; Daniel Walker Howe, *The Political Culture of the American Whigs* (Chicago, 1979); and Howe, "Evangelical Movement and Political Culture."

⁸⁰ Keith Thomas, "The Brilliant Misfit," *New York Review of Books*, 37 (June 14, 1990): 47.

⁸¹ Gene Wise, *American Historical Explanations: A Strategy for Grounded Inquiry* (Homewood, Ill., 1973), esp. 113-57. Wise also explained clearly why New Left historians misunderstood counter-Progressive historians (such as Benson) to be conservatives who favored "consensus history," see, for example, pp. 92-93.

relatively new enterprises as Gay and Lesbian History, Men's History (two entries), and Sexuality (four entries)—but there was no category for “Historiography.” For most historians, the historiography practiced in an introductory chapter or paragraph is frequently self-promoting, a result encouraged by incentives to graduate students and job-seeking assistant professors to be *original*.⁸² The norms of the profession, in short, encourage frequent reinvention of the wheel, a tendency that cannot be eliminated but could be restrained. Labeling historians or grouping them into “schools” needs to be not the end but the beginning of discussion.

⁸² Well-established historians also may succumb to this pressure: Robert M. Collins, “The Originality Trap: Richard Hofstadter on Populism,” *Journal of American History*, 76 (June 1989): 150–67.

Review Article
The Contact of Cultures:
Perspectives on the Quincentenary

IDA ALTMAN and REGINALD D. BUTLER

WHETHER REGARDED AS AN OPPORTUNITY to reach a wider public than usually takes an interest in events remote in time and place, an occasion for scholarly reflection and discussion, or an opportunity to air grievances both historical and contemporary, the Columbian Quincentenary elicited a range of institutional, individual, and collective responses. There were symposia, exhibitions, publications, debates, demonstrations, the multimillion-dollar construction of a lighthouse, even an automobile rally. Depending on one's expectations, the activities connected with the approach and observance of the Quincentenary might have proved heartening, frustrating, disappointing, or absurd. In its aftermath, members of the scholarly community who have not done so before would do well to consider the relevant scholarship that has emerged, generally with considerably less fanfare than other aspects of the commemoration.¹ The scholarly work that appeared during the few years leading up to and culminating in the 500th anniversary of the first Columbian voyage is notably solid, subtle, varied, and stimulating. The best of it obviously is the product of long-term research and thought. This scholarship not only offers significant new bodies of material but also utilizes methods of analysis from a range of disciplines (anthropology, history, art history, literature) that at least in some realms of inquiry (the study of contacts among

The ideas presented here in some respects represent a continuation of discussions conducted during an institute sponsored by the National Endowment for the Humanities in Madison, Wisconsin, in the summer of 1989. We would like to acknowledge our fellow participants and the visiting lecturers in the institute; we also thank John Hébert of the Library of Congress for drawing us directly into consideration of the Quincentenary and its implications and for assisting with the illustrations.

¹ For a good survey of the recent literature related to Quincentenary themes, see the last chapter ("Beyond 1992") in James Axtell, *Beyond 1492: Encounters in Colonial North America* (Oxford, 1992). The essays in *Beyond 1492* reflect many years of research by one of the most influential scholars in the field of North American ethnohistory; they offer a stimulating introduction to the field as well as a good indication of Axtell's ideas and work. See also Kenneth Maxwell, "¡Adiós Columbus!" in *New York Review of Books*, January 28, 1993. Maxwell's emphasis on the significance of Portuguese activities is useful, although his observation on "how narrow and self-contained much contemporary scholarship has become" fails to place recent work in its appropriate historiographical context. David P. Henige's *In Search of Columbus: The Sources for the First Voyage* (Tucson, Ariz., 1991) (which Maxwell does not review), for example, while focusing on one text (the so-called *Diario* of the first voyage) and how it has been used and misused, has implications not only for virtually every study dealing with Columbus but also for how to approach and use essential texts that may be available only in partial and altered forms. For another review of some important recent works, see J. H. Elliott, "The Rediscovery of America," *New York Review of Books*, June 24, 1993.

Kenneth J. Andrien and Rolena Adorno, eds., **Transatlantic Encounters: Europeans and Andeans in the Sixteenth Century** (Berkeley: University of California Press, 1991).

James Axtell, **Beyond 1492: Encounters in Colonial North America** (New York: Oxford University Press, 1992).

Philip Boucher, **Cannibal Encounters: Europeans and Island Caribs, 1492–1763** (Baltimore: Johns Hopkins University Press, 1992).

Louise M. Burkhart, **The Slippery Earth: Nahua-Christian Moral Dialogue in Sixteenth-Century Mexico** (Tucson: University of Arizona Press, 1989).

Felipe Fernández-Armesto, **Before Columbus: Exploration and Colonisation from the Mediterranean to the Atlantic, 1229–1492** (London: Macmillan Education, 1987).

Stephen Greenblatt, ed., **New World Encounters** (Berkeley: University of California Press, 1993).

James Lockhart, **The Nahuas after the Conquest: A Social and Cultural History of the Indians of Central Mexico, Sixteenth through Eighteenth Centuries** (Stanford, Calif.: Stanford University Press, 1992).

Sabine MacCormack, **Religion in the Andes: Vision and Imagination in Early Colonial Peru** (Princeton, N.J.: Princeton University Press, 1991).

Carolyn Merchant, **Ecological Revolutions: Nature, Gender, and Science in New England** (Chapel Hill: University of North Carolina Press, 1989).

Richard Price, **Alabi's World** (Baltimore: Johns Hopkins University Press, 1990).

Timothy Silver, **A New Face on the Countryside: Indians, Colonists, and Slaves in South Atlantic Forests, 1500–1800** (Cambridge, Eng.: Cambridge University Press, 1990).

John Thornton, **Africa and Africans in the Making of the Atlantic World, 1400–1680** (Cambridge, Eng.: Cambridge University Press, 1992).

David J. Weber, **The Spanish Frontier in North America** (New Haven, Conn.: Yale University Press, 1992).

Samuel M. Wilson, **Hispaniola: Caribbean Chiefdoms in the Age of Columbus** (Tuscaloosa: University of Alabama Press, 1990).

Europeans, Americans, and Africans in the aftermath of the Columbian voyages being one) laudably have become less and less artificially separate.

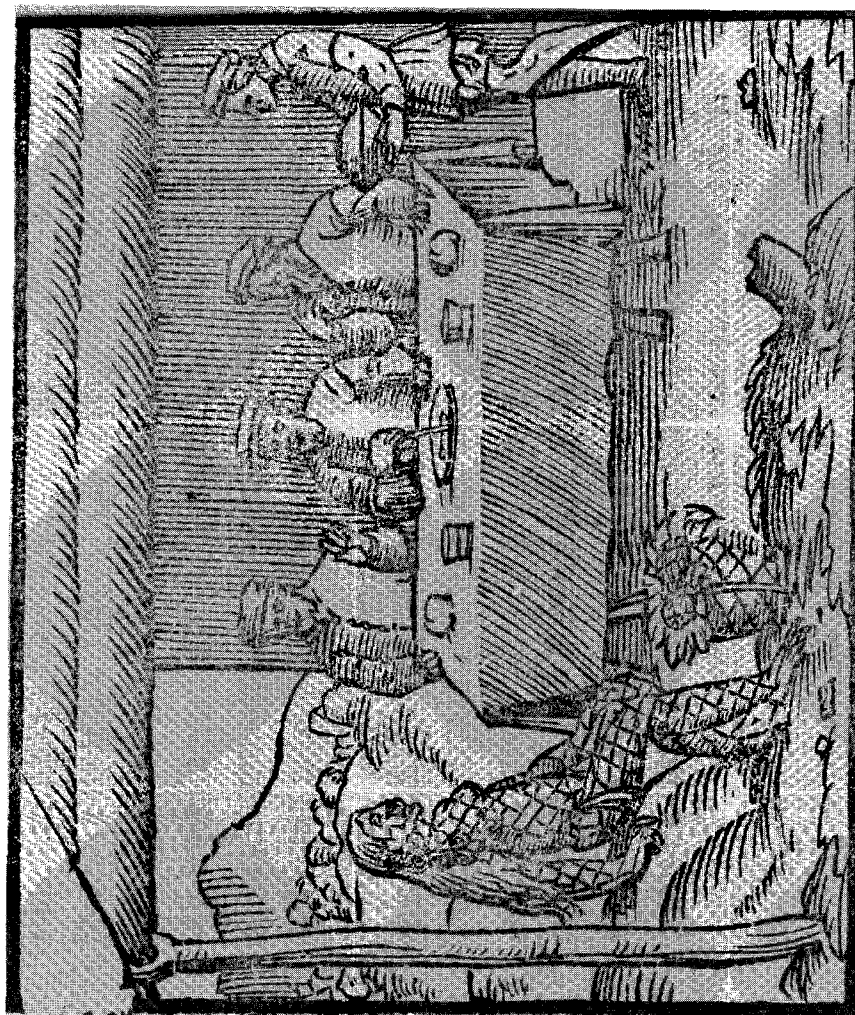
We propose to consider here what can only be called a sampling of the very recent literature that bears most directly on one of the key themes of the Quincentenary: the substance and meaning of contacts among the peoples who,

willingly or not, came together because of the undertaking of Columbus and those who accompanied or followed him.² While diverse in topic and method, this literature either treats some aspect of the process of contact or suggests approaches by which to do so, mainly with regard to the use (and sometimes reexamination) of texts. For the early years of European experience in the Americas, texts are far from plentiful, and those written at the outset were almost invariably the product of European pens and minds. Most of the work mentioned here takes as its focus early periods of contact; yet it seems necessary, especially for comparative purposes, to construe the contact period broadly, given both the chronological disparities and the sometimes parallel, sometimes convergent trajectories of European activities in Africa, the Atlantic, and the Americas. It should be noted that, with one exception, the studies cited were published from 1989 on, an arbitrary date but one that coincides with the increasing attention paid to the Quincentenary. We wish to emphasize the richness of these works, which certainly merit much more detailed individual consideration than the present essay allows.

"Contact" in the post-Columbian era is often taken to refer mainly and even exclusively to the interactions of Europeans and indigenous Americans. But our intention is to bring into conjunction the multiple contacts (any kind of direct interaction between peoples, whether peaceful or hostile, continuing or sporadic) that took place in the Atlantic world chiefly as a result of the expansion of European commercial and colonizing activities beginning in the latter part of the fifteenth century. The simultaneous consideration of relationships between Europeans and Africans (on both sides of the Atlantic), Africans and Indians, and, not least, the groups that participated actively in commercial and colonial endeavors in the Atlantic world can illuminate the terms and nature of European-indigenous relations as well as expand and qualify the context in which those relations developed. Furthermore, the movement of Europeans into the Atlantic islands and then the Caribbean and American mainland was, perforce, a movement of Africans as well. If Europeans provided the main impetus, Africans over time provided disproportionate numbers of the personnel that staffed (and hence made possible) many European-directed enterprises in the Atlantic world. Involuntary collaborators though they were, the Africans who participated in the European colonization of the Americas added significantly to the complexity in social relations, with major consequences for the formation of new societies and for the transformation of the land.

On the whole, the scholarship we cite emphasizes analysis of indigenous American or African cultures and their responses to the new and changing circumstances of contact and exploitation precipitated by European activities. This focus reflects efforts to redress existing imbalances in the historiography of particular areas and societies, as James Lockhart explains he set out to do when he

² For an earlier suggestive treatment of similar themes relating to contact and settlement in the British colonies, see T. H. Breen, "Creative Adaptations: Peoples and Cultures," in Jack P. Greene and J. R. Pole, eds., *Colonial British America: Essays in the New History of the Early Modern Era* (Baltimore, Md., 1984). He reviews much of the relevant literature to that date.



European men observing an Indian woman, from Girolamo Benzoni, *Historia del Mondo Nuovo* (Venetia, 1572). Benzoni lived and traveled extensively in the Caribbean and other parts of Spanish America in the sixteenth century. Rare Book and Special Collections, Library of Congress.

first began to study the Nahuas of central Mexico.³ Yet also implicit (and sometimes quite explicit) in these works is the assumption that one cannot make full sense of the process and outcome of contact between cultures without a thorough understanding of all the actors involved.⁴ Richard Price develops this approach extensively in *Alabi's World*, incorporating the narratives of the Sarakmaks (maroons) of Suriname, the diaries of the Moravian missionaries who lived among them in the late eighteenth and early nineteenth centuries, and the journals and letters of Dutch colonial officials.

THE HISTORY OF CONTACT AS SUCH began with the commercial, maritime, and sometimes military expansion of those European groups—Italians (mainly Genoese), Portuguese, and Castilians (and, sporadically, the French)—who in the late Middle Ages began shifting their focus of attention from the Mediterranean to the Atlantic.⁵ The question of why it was Europe rather than some other region that began to expand aggressively in the late Middle Ages is often raised, given the existence of comparable navigational development elsewhere. Among the world regions adjacent to the Atlantic, however, commercial and maritime activity had developed most intensely in Mediterranean and North Atlantic Europe. While a West African coastal trade did exist, both the internal marketing routes and the commercial networks that linked West Africa to the Mediterranean operated overland.⁶

The westward expansion from the Mediterranean to the Atlantic hinged primarily on individual and collective ambition for financial gain and commercial advantage. In this respect, the colonizing and commercial enterprises of the thirteenth through fifteenth centuries in the Mediterranean and Atlantic traced by Felipe Fernández-Armesto are significant.⁷ Patterns of rivalry and competition

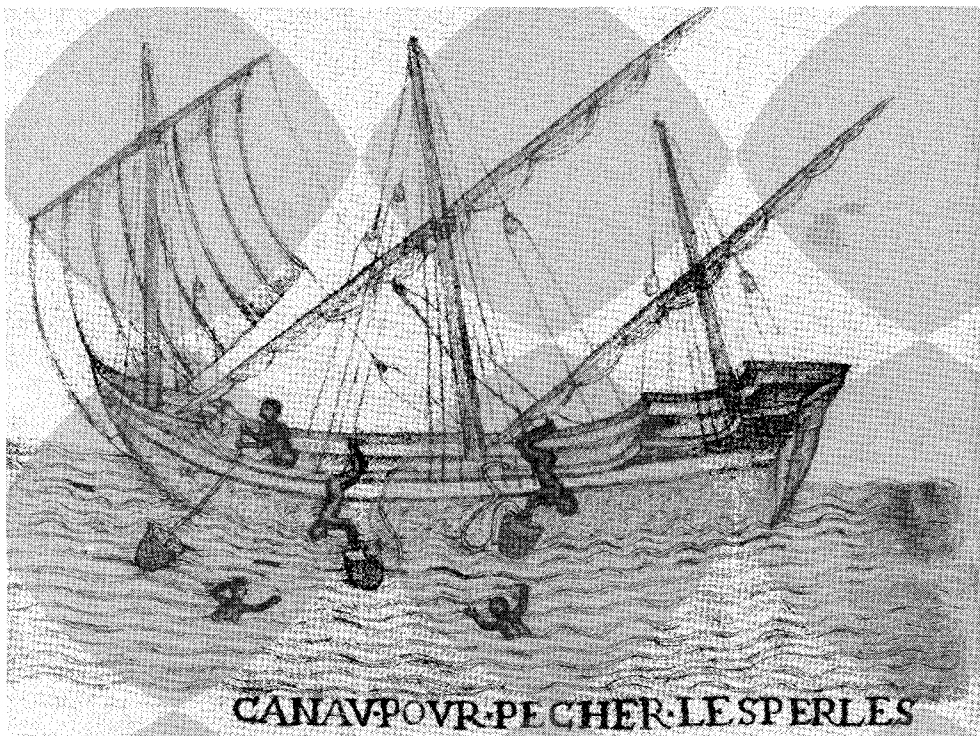
³ James Lockhart, *The Nahuas after the Conquest* (Stanford, Calif., 1992), 1, 427. Lockhart's study, the culmination of years of research in Nahuatl documents generated by the Nahuas themselves, marks a significant step toward amending the previous historiographical emphasis on Spanish institutions and society; it also, by revealing basic organizational patterns of Nahua society and culture, provides a new basis for assessing the process of adjustment and adaptation between the two.

⁴ See, for example, Carolyn Merchant, *Ecological Revolutions: Nature, Gender, and Science in New England* (Chapel Hill, N.C., 1989), Chapter 4, "The Animate Cosmos of the Colonial Farmer." See also Sabine MacCormack's discussion of the Christian conception of demonic power and its implications for the Spaniards' understanding of Andean religion in "Demons, Imagination, and the Incas," Stephen Greenblatt, ed., *New World Encounters* (Berkeley, Calif., 1993).

⁵ Felipe Fernández-Armesto, *Before Columbus: Exploration and Colonisation from the Mediterranean to the Atlantic, 1229–1492* (London, 1987), discusses in detail the colonizing and commercial activities of the Iberian states and the Genoese in the Mediterranean and the Atlantic, culminating in the protracted conquest and settlement of the Canary Islands, which lasted nearly to the end of the fifteenth century.

⁶ John Thornton, *Africa and Africans in the Making of the Atlantic World, 1400–1680* (Cambridge, 1992), 21–22, suggests that, like Europeans, both West Africans and Caribbean islanders were capable of taking to the open sea (see also pp. 14–26, 36–42). On contemporary maritime activity in the Indian Ocean, see John E. Wills, Jr., "Maritime Asia, 1500–1800: The Interactive Emergence of European Domination," *AHR*, 98 (February 1993): 83–105. The work of John Middleton, *The World of the Swahili: An African Mercantile Civilization* (New Haven, Conn., 1992), on East Africa raises the question of why comparable commercial consolidation did not take place in West Africa in response to the opportunities of the Atlantic trade.

⁷ Margarita Zamora writes of Columbus's 1493 letter, which she translated from Antonio Rumeu de Armas's transcription, that "it represents the Discovery unequivocally as a joint commercial



Pearl divers, from Sir Francis Drake, *Histoire naturelle des Indes*, early 1590s. The Spanish used Indians to dive for pearls in the early years in the Caribbean, but the divers pictured here are clearly African. The Pierpont Morgan Library, New York, MA 3900, f. 57.

as well as sporadic cooperation among European states and entrepreneurs and of collaboration between private and state interests emerged clearly in that period. All of these figured prominently in or shaped European activities in Africa and colonizing efforts in the Americas, with particular consequences for the people encountered there.

The fragmentation of European efforts and the rivalries and conflicts among groups and individuals, whether of the same nationality or not, remind us that, notwithstanding the convenience and often the necessity of relying on such categories as "Europeans," "Africans," and "Indians" in certain contexts, even the most homogeneous of these groups, the Europeans, were hardly unified.⁸

venture between an individual who was driven by personal worldly and spiritual ambitions and the state that was contractually bound to compensate him for the initiative and would benefit from his efforts." Zamora, "Christopher Columbus's 'Letter to the Sovereigns': Announcing the Discovery," in Greenblatt, *New World Encounters*, 3. See Fernández-Armesto, *Before Columbus*; and Louis Montrose on Walter Raleigh's motivations and ambitions in "The Work of Gender in the Discourse of Discovery," in Greenblatt, *New World Encounters*. The articles in this volume, most of which have appeared previously in *Representations*, focus on aspects of the meaning and consequences of European activities in the Americas.

⁸ See the discussion in Breen, "Creative Adaptations," 198–201, of the diversity within these groups. Breen emphasizes that, in the early phases of colonization, the representatives of specific localized cultures came into contact with one another. He does suggest, however, that "social divisions notwithstanding . . . English colonists possessed a greater awareness of a shared culture, especially a

Although the aspirations of various European groups traveling to Africa or the Americas were usually similar, the very effort of trying to realize these aspirations generated competition and factionalism, while at the same time rivalries may have intensified the sense of national identity. Hence Columbus clashed with many of the Spaniards who participated in his expeditions (there were, of course, those who allied with him and his family as well), and groups of Europeans vied for advantage and control almost everywhere—the French and Portuguese in Brazil, the French, Spanish, and English along the Atlantic seaboard of North America, and just about everyone in the Caribbean. Even where Spanish control was essentially unassailable, factionalism erupted, as seen in the bitter civil strife in early Spanish Peru.⁹ Spain's early and successful occupation of the most populous and wealthy regions of the Americas doubtless exacerbated competition among the European groups seeking a toehold there and made Spain and its possessions the major object of other Europeans' aspirations. Louis Montrose suggests that in Walter Raleigh's *Discoverie* the Spaniard "is, at once, an authority to be followed, a villain to be punished, and a rival to be bested" and notes the contemporary "embarrassment of England's cultural and imperial belatedness."¹⁰

Cooperation, or at least toleration of activities of other Europeans, did occur at times. Where no single group of Europeans could claim an exclusive sphere of influence, people had no choice but to accept the presence of others. In Africa, for example, even the Portuguese, the group first and best established and the only one to create an actual colony in the sixteenth century, were unable to exercise sufficient control over African states to dictate the terms of their dealings with other Europeans or even to control the activities of private citizens.¹¹ In the North

common language, than did either the Africans or the Indians," p. 201. On the articulation and development of societies in Anglo-America, see also Jack P. Greene, *Pursuits of Happiness: The Social Development of Early Modern British Colonies and the Formation of American Culture* (Chapel Hill, N.C., 1988).

⁹ This kind of conflict was not, however, very destructive, as James Lockhart demonstrated in *Spanish Peru, 1532–1560: A Colonial Society* (Madison, Wis., 1968), a revised edition of which is now available. As John Guilmartin writes, "Whatever their internal differences and whatever their proclivity to settle them with the sword, the Spanish owed allegiance to the same emperor and the same church on terms that all understood. Their long-term strategic objectives were the same: to carve the Inca domains, extract wealth from the Andean communities, and convert the indigenous peoples to Catholicism." Guilmartin, "The Cutting Edge: An Analysis of the Spanish Invasion and Overthrow of the Inca Empire, 1532–1539," in Kenneth J. Andrien and Rolena Adorno, eds., *Transatlantic Encounters: Europeans and Andeans in the Sixteenth Century* (Berkeley, Calif., 1991), 59–60.

¹⁰ Louis Montrose, "The Work of Gender," in Greenblatt, *New World Encounters*, 193.

¹¹ See Thornton, *Africa and Africans*, 38, 57–66. We have used Thornton's survey of West African history principally because it deals extensively with the period of interest and because it attempts to bring into connection the West African experience on both sides of the Atlantic and examine the impact of African participation in the forging of American societies. Other recent works on West Africa explore the nature of European activity in the region but do not consider African activity in the Americas; see, for example, Joseph C. Miller, *Way of Death: Merchant Capitalism and the Angolan Slave Trade, 1730–1830* (Madison, Wis., 1988); Barbara L. Solow, ed., *Slavery and the Rise of the Atlantic System* (Cambridge, Mass., 1991); and Robin Law, *The Slave Coast of West Africa, 1550–1750: The Impact of the Atlantic Slave Trade on an African Society* (Oxford, 1991). Miller emphasizes the role of Angolan political and commercial elites and others in the development of the Atlantic system. These works focus primarily on the eighteenth century, although there is some treatment of the earlier period (see, for example, William D. Phillips, Jr., "The Old World Background of Slavery in the Americas," in Solow, *Slavery*). See also Ivana Elbl, "The Horse in Fifteenth Century Senegambia," *International Journal of African Historical Studies*, 24 (1991): 85–110.

Atlantic, fishermen and whalers of many nationalities frequented the Newfoundland coast after 1500. In the Caribbean, both the British and the French colonized the island of St. Christopher for a time, allied against the Spanish and the Caribs.

With fragmentation and conflict more the rule than the exception in relations among Europeans, local people could decide how they might best pursue their own objectives by playing off groups of rival Europeans against each other. John Thornton's argument that Africans participated in the slave trade as the equal and uncoerced partners of Europeans implies that the entrance of more groups of Europeans into the trade, if anything, enhanced African control and choice; the lucrative fur trade of North America that attracted the French, English, and Dutch seems to have offered similar leverage to native participants, at least in the early years of European settlement. James Axtell suggests that Indians of eastern North America "were shrewd enough to advance their own bargaining position by playing European competitors against each other, by avoiding superfluities that had no place in their own culture, and by being extremely finicky about the quality and style of goods they would accept." Axtell's discussion of the nature of those goods raises interesting points of comparison to West Africa. There, Thornton contends, the European trade mainly brought luxury goods rather than necessities, and "early African manufacturing was in many ways quite capable of providing for the continent's needs." One could claim the same for the Americas—after all, people do not need things they have never seen or used—but basic self-sufficiency does not preclude the substitution of items that may be stronger, more efficient, cheaper, or more prestigious.¹²

The importance of indigenous allies in French-Portuguese conflicts in Brazil or British-French conflicts in North America is well known, but the outcome or resolution of such struggles usually favored one group or another of Europeans more than it did their allies. Philip Boucher traces the shifting fortunes of the Caribs, who hated the Spanish and for the most part favored the French over the British in the seventeenth and eighteenth centuries. Allying with the French may have brought the Caribs some relief in the short term, but the goal of both the French and British was nonetheless the same—to bring the islands under their own control in order to exploit them for commercial agriculture, a goal that meant the eventual elimination of the Caribs from the Lesser Antilles. In the end, for the Caribs it scarcely mattered with whom they had allied.¹³

Africans and Americans were frequently affected by the limited control

¹² See Thornton, *Africa and Africans*, 6–7, 44; and James Axtell, "The First Consumer Revolution," in *Beyond 1492*, 132. Axtell writes that "the earliest items favored by both native men and women were metal tools to make their work go easier and faster. Since the natives were already fully equipped with the requisite tools to manage their environment, they purchased the same kinds of European implements made of superior materials," p. 135. He also notes that, "from the earliest indirect contact with Europeans, the Indians sought to enhance their beauty and status with decorations of foreign material or manufacture," p. 139, a situation again reminiscent of the West African one.

¹³ Philip P. Boucher, *Cannibal Encounters: Europeans and Island Caribs, 1492–1763* (Baltimore, Md., 1992), esp. Chapter 4, "'As if no such people existed': Island Caribs in Decline, 1689–1763." He remarks that "great power hostilities . . . helped Island Caribs in the short run to maintain a measure of independence at Dominica and St. Vincent," p. 94.



Outina against Potanou, from Charles La Ronciere, *La Floride française: Scènes de la vie Indienne peintes en 1564* (1564; Paris, 1928). The scene shows the French participating in a battle between indigenous groups. Rare Book and Special Collections, Library of Congress.

European states could exercise over enterprises developing hundreds or thousands of miles away and the concomitant tendency for Europeans to act as free agents, even when officially or nominally acting on behalf of their home governments. This kind of fragmentation sometimes enhanced African leverage in commercial affairs; after all, individual Europeans were even less likely than European states to be capable of exercising effective force and imposing unequal relations. In the Americas, however, the tendency at many levels to repudiate or ignore authority more often had the opposite effect, fomenting turmoil and destruction. In Hispaniola, for example, Columbus appears to have responded to the actions of ostensible subordinates post hoc rather than setting and carrying out his own policies (which admittedly might not have been much milder).¹⁴ And Cortés, the archetypical freelancer, had to confront the chaos set off by Pedro de Alvarado's unprovoked attack on the people of Tenochtitlan during his ab-

¹⁴ See Samuel M. Wilson, *Hispaniola: Caribbean Chiefdoms in the Age of Columbus* (Tuscaloosa, Ala., 1990), 83.

sence.¹⁵ Competition among rivals could heighten rather than neutralize demands placed on coerced populations.¹⁶

Indeed, official policies regarding relations with local people developed by home governments and colonial administrations tended to be more moderate than the choices made by either officials or private individuals on the scene.¹⁷ Conflict between governing officials itself could generate spiraling exploitation, as everyone tried to grab whatever he could in the absence of effective restraining authority.¹⁸ Those individuals who, like the Portuguese settlers in West Africa, established friendly relations with the local people, such as the *coureurs des îles*, who "knew Carib languages, customs and terrain," could act as advance agents of colonization. Boucher writes that "these traders' knowledge and toughness also facilitated French invasions of Carib strongholds."¹⁹ The role played by missionaries was comparably ambiguous, for, ultimately, their aim of converting and "civilizing" native peoples fit well within the more general objectives of colonization, even if their methods and priorities differed in the specifics.

UNTIL FAIRLY RECENTLY, COLUMBIAN "DISCOVERY" LITERATURE focused primarily on the significance and repercussions of the opening of the Americas for the intellectual life, society, and economy of Europe.²⁰ Scholars have by no means abandoned such topics, although they are now more likely to qualify their approach to them by taking into account the growing body of work on indigenous experience and response.²¹ Much recent scholarship moves beyond the assump-

¹⁵ See Inga Clendinnen, "'Fierce and Unnatural Cruelty': Cortés and the Conquest of Mexico," in Greenblatt, *New World Encounters*.

¹⁶ Wilson writes that in 1498 the cacique Guarionex "was still required to deliver the same quarterly tribute of food and gold to the Adelantado; in addition the anti-Colón forces of Roldán were staying in the villages of the Vega and probably demanding an equal amount of food and gold for themselves," *Hispaniola*, p. 102.

¹⁷ Boucher, *Cannibal Encounters*, 45. He notes that in England, "by 1670, at the request of the Privy Council, the new Council on Plantations directed all colonial governors to prevent settler provocations against the Indians and to punish severely the perpetrators," p. 64. Nonetheless, at the end of 1674, former Deputy-Governor Thomas "Indian" Warner of Dominica was treacherously murdered by the Antiguan militia along with sixty or seventy of his followers (see pp. 67, 77–82). See also the accounts included in Peter Hulme and Neil L. Whitehead, eds., *Wild Majesty: Encounters with Caribs from Columbus to the Present Day* (Oxford, 1992), 89–106.

¹⁸ A good example of this kind of situation was the furor caused by the arrival of the notoriously venal first Audiencia (high court) in New Spain in the mid-1520s, which the Spanish crown doubtless hoped would curb the power and influence of Cortés. The Audiencia judges, especially the president, Nuño de Guzmán, pillaged the colony. See Rolena Adorno's discussion of Cabeza de Vaca's (contrasting) peaceful efforts at conversion in "The Negotiation of Fear in Cabeza de Vaca's *Naufragios*," in Greenblatt, *New World Encounters*, esp. 69–78. She notes, "In Cabeza de Vaca's narrative as in Oviedo's, the most dramatic confrontation was that which took place between countrymen, between Cabeza de Vaca's group and the Spanish slave hunters," p. 74.

¹⁹ Boucher, *Cannibal Encounters*, 52.

²⁰ See, for example, many of the articles in Fredi Chiappelli, ed., *First Images of America: The Impact of the New World on the Old*, 2 vols. (Berkeley, Calif., 1976); J. H. Elliott, *The Old World and the New, 1492–1650* (Cambridge, 1970); Hugh Honour, *The New Golden Land: European Images of America from the Discoveries to the Present Time* (New York, 1975). Honour chronicles the depiction of America and its people in European art and the impact of particular interpretations or representations on Europeans' perception of the New World. See also Bernadette Bucher, *Icon and Conquest: A Structural Analysis of the Illustrations of de Bry's "Great Voyages"* (Chicago, 1981).

²¹ But not always; compare Mary C. Fuller's "Raleigh's Fugitive Gold: Reference and Deferral in

tion of the unknowability of the Other,²² taking as its main point of departure texts and other forms of expression generated directly or indirectly by indigenous authors.²³ The use of such material requires subtlety and sophistication. David Damrosch, discussing the pitfalls of trying to classify Nahuatl songs as pre-conquest or post-conquest, suggests that "we have to deal with a continual uncertainty as to whether a given poem responds to events of 1460 or events of 1560—if not to both at once."²⁴ Where indigenous texts are lacking, some scholars have combined information gleaned from texts produced by Europeans with other evidence that is indigenous in origin.²⁵ Sabine MacCormack reminds us in *Religion in the Andes* that "both the structure and the argument of the book arise from the inescapable fact that whatever understanding it may now be possible to gain of Inca and Andean religion must be channeled through the writings of men who in one way or another were outsiders to that religion."²⁶ The literature resulting from innovative uses of texts and other sources is becoming so substantial and on the whole is so impressive that here we can only suggest some of the main commonalities that emerge from it.

Indigenous Americans everywhere, confronted with the arrival of Europeans—whether in force or in unimpressive numbers—sought to explain, assimilate, and incorporate the newcomers (and the objects and concepts they brought with them) into existing cultural and social categories. What to Europeans might have seemed bizarre and inexplicable initial responses to their presence were, from the indigenous point of view, logical and sensible. Rolena Adorno, for example,

The Discoverie of Guiana," which offers an entirely literary analysis of the text, to Montrose's discussion of the same text, which reflects the historical context of Raleigh's aspirations and their limitations; both in Greenblatt, *New World Encounters*.

²² In his conclusion to *The Nahuas*, Lockhart writes: "To the concept [of the Other] I would have little objection, if it is taken as encouraging us to study the extent to which groups newly in contact view each other as sharing basic common traits or not—whether they identify each other as within the pale or without. But the approach seems to lead to the notion that the groups facing each other are absorbed with this question and furthermore that they generally view each other as radically distinct," p. 444. With regard to the Nahuas and Spaniards, Lockhart contends there that "each side remained essentially more concerned with its own internal affairs and conflicts than with understanding the other." Nonetheless, the question of the perception of the Other has stimulated significant scholarly inquiry and the use of innovative methodologies over the past decade. It may have been one of the crucial factors that in the long term shaped legal, social, and economic relations among the diverse groups of the Americas. See the comments of Elliott, "Rediscovery," 38.

²³ Louise M. Burkhart, *The Slippery Earth: Nahua-Christian Moral Dialogue in Sixteenth-Century Mexico* (Tucson, Ariz., 1989), is based on her analysis of catechistic literature in Nahuatl produced by members of the Mendicant orders in New Spain in collaboration with Nahua informants, whereas Lockhart's main sources for *The Nahuas* are mundane documents (wills, land grants, town council records), annals, and other texts produced by Nahuatl speakers.

²⁴ David Damrosch, "The Aesthetics of Conquest: Aztec Poetry before and after Cortés," in Greenblatt, *New World Encounters*, 140.

²⁵ In *Hispaniola*, Wilson combines his reading and analysis of Las Casas, Oviedo, and Peter Martyr with archaeological evidence from the Greater Antilles. Sara Castro-Klarén, in "Dancing and the Sacred in the Andes," uses not a contemporary text but rather a modern short story (by Peruvian anthropologist and author Jose María Arguedas) as the basis for her discussion of the survival and transformation of the beliefs of the Taqui-Oncoy, a cult that sought "to empower the ancient local gods with the force and the discourse necessary to compete with, and eventually, to defeat the invading god, the Spaniards, and the pestilence that followed in their wake." Castro-Klarén, in Greenblatt, *New World Encounters*, 163.

²⁶ Sabine MacCormack, *Religion in the Andes: Vision and Imagination in Early Colonial Peru* (Princeton, N.J., 1991), 5.

argues that the inference by local people that Europeans came from the sky (found in the Cabeza de Vaca account) implies not the perceived divine or "heavenly" provenance of the intruders, as is commonly assumed, but rather the Indians' lack of reliable information on true European origins; she suggests that other such allusions to "heavenly" origins may have been similarly misinterpreted.²⁷

Bearing in mind that, for the earliest recorded contacts, most of the near-contemporary accounts come from Europeans, there does exist a fair amount of agreement nonetheless that the people who first greeted Europeans in the Americas often suspected that Europeans might be, if not divine, then at least spiritually powerful beings. Axtell writes that, "unlike the Europeans, the natives expected novel strangers to be either equal or superior to themselves, either powerful and potentially dangerous 'persons' animated by living 'souls' like their own, or 'gods,' 'spirits' from the heavens whose powers were of a higher order."²⁸ Samuel Wilson suggests a similar caution in the dealings of the Taínos with Columbus and his people.²⁹ Treating Cabeza de Vaca as a shaman made sense in indigenous terms,³⁰ as did the extension of hospitality to Europeans in the form of food, tobacco, gifts, and even women. Yet the presence of generous hospitality suggesting a warm and friendly welcome by no means proves an irrational or childlike credulity on the part of native groups. Cabeza de Vaca's success as a shaman hinged both on the success of his "cures" and the size of his entourage; he and his companions moved from place to place accompanied by a large, intimidating group.³¹ Whenever the intruders' behavior was sufficiently treacherous or violent to overcome any lingering uncertainty over their real nature, Indians readily revised their assessment of the newcomers and tried to avoid or resist them. David J. Weber relates an anecdote from the expedition of Hernando de Soto, which he justifiably terms "a scourge upon the land": "To gain the help of a group of powerful Natchez Indians, De Soto sent word that he was 'the son of the sun.' He may have known that rulers of the Natchez theocracy claimed to be descended from the sun . . . But a Natchez chief put De Soto to the test, telling him to 'dry up the great river and [then] he would believe him.'"³² Accounts of

²⁷ Adorno refers to a passage in the 1542 edition of the narrative, which she translates as follows: "Among all these peoples, it was held for very certain that we came from the sky, because about all the things that they do not understand or have information regarding their origins, they say that such phenomena come from the sky." Rolena Adorno, "Negotiation of Fear," in Greenblatt, *New World Encounters*, 68, see also 62.

²⁸ James Axtell, "Native Reactions to Invasion," in *Beyond 1492*, 101. He develops the same concept in "Imagining the Other," also in *Beyond 1492*, 33: "The Indians of North America shared a belief that all living things possessed 'souls' or 'spirits' capable of unrestricted movement in time and space and harming or helping other 'persons.' Because the magnitude of their power was largely unknown and because they might appear in a strange guise, each 'person' had to be treated with respect and circumspection."

²⁹ "During the contact period the Taíno elite were uncertain about where the foreigners should be treated in the indigenous system of social and political ranking: Were they equivalent to commoners, elites, or gods?" Wilson, *Hispaniola*, 137.

³⁰ See Adorno, "Negotiation of Fear"; and Axtell, *Beyond 1492*, 38–39.

³¹ Adorno writes that "the following of native roads, the accompaniment by native groups, the advance messengers who foretold their arrival and arrived at an understanding with the new village as to the reception demanded of them, made possible the remarkable journey whose wonders Cabeza de Vaca often would repeat," "Negotiation of Fear," p. 63.

³² David J. Weber, *The Spanish Frontier in North America* (New Haven, Conn., 1992), 50, 54. Weber here is quoting from James A. Robertson, ed. and trans., *True Relations of the Hardships Suffered by*

that expedition are also replete with incidents in which native guides led the Spaniards astray, presumably with the intention of getting rid of them as quickly as possible.

In Hispaniola, within three years of Columbus's first landing on the island, Indian attitudes changed from friendliness to suspicion and hostility.³³ This quick transition, together with other episodes in which the *immediate* response of local people to European newcomers was hostile, underscores not only how little time it could take before people in the Americas grasped the real intentions and nature of European intruders but also how often apparent first contacts were not first. Earlier contacts that had gone sour, often not recorded by Europeans or not communicated between rival groups of Europeans, set a negative precedent for subsequent ones, as did indirect knowledge of European activities gained from groups who had firsthand experience of them.³⁴ Weber writes that "the Spaniards' reputation, already established by the Narváez expedition, had preceded them, so that Indians in De Soto's path often resisted his incursions by abandoning their towns and employing guerilla tactics or, on occasion, executing massive assaults."³⁵

Despite the rapidity with which relations often soured, the nature of friendly (or at least non-hostile) interactions is of interest, since these reflected indigenous categories and priorities and continued to figure in relations with Europeans even after they began to deteriorate. Early non-hostile encounters frequently involved exchanges of gifts and demonstrations of hospitality by both sides, in many cases leading to the establishment of trade relations. They also produced alliances, in which Europeans might support a group against its traditional enemies or rivals. (When conflicts involved rival groups of Europeans, the alliances made usually reflected already-existing antagonisms between indigenous groups.) In these kinds of interactions, indigenous objectives and perceptions were paramount, even if in the end Europeans were able to turn them to their advantage.

The giving of gifts as part of initial friendly overtures was clearly as well established a practice among Indians as among Europeans, although, as Axtell points out, the intentions most likely were different. He notes that "the newcomers tended to see gifts as bribes . . . In native America, by contrast, gifts were at

Governor Fernando de Soto and Certain Portuguese Gentlemen during the Discovery of the Province of Florida, 2 vols. (De Land, Fla., 1933).

³³ See Wilson, *Hispaniola*, 74–75, and following for the conflict in the Vega. Conflict obviously began before Columbus returned on his second voyage to discover the destruction of the fort at Navidad and disappearance of the people he had left behind.

³⁴ Axtell notes that "second contacts are easily confused with truly first contacts because they are the initial encounters described in several famous documents by European colonizers," *Beyond 1492*, p. 104. It is worth remembering that there exists a corresponding, if somewhat different, problem in trying to pinpoint "first" contacts between Europeans and Africans. Thornton argues that aspects of European culture—Christianity in particular—had begun to exert a perceptible influence in some parts of West Africa even before the transatlantic slave trade was established; hence African slaves brought to the Americas did not necessarily "encounter" European culture there for the first time, and this earlier influence figured in some of their responses to contact with Europeans in the Americas.

³⁵ Weber, *Spanish Frontier*, 51. Cabeza de Vaca was one of the handful of survivors of the disastrous expedition of Pánfilo de Narváez to Florida. Hernando de Soto also traveled through areas on the east coast where Lucas de Ayllón had been.

once 'words' in the rich metaphorical language of political councils and sureties for one's words."³⁶ Inga Clendinnen suggests that "Moctezoma's gifts [to Cortés] were statements of dominance, superb gestures of wealth and liberality made the more glorious by the arrogant humility of their giving: statements to which the Spaniards lacked both the wit and the means to reply."³⁷ It seems that, whether intended as an expression of welcome, respect, or superiority, indigenous gift-giving was more heavily imbued with symbolic meaning than was the pragmatic European dispensing of items chosen in the hope that they would yield the greatest return for the least expenditure.³⁸ What Lockhart calls "double mistaken identity" may be relevant to the perceived meaning and consequences of bestowing gifts. He defines this concept as a "process . . . whereby each side takes it that a given form or concept is essentially one already known to it, operating in much the same manner as its own tradition, and hardly takes cognizance of the other side's interpretation."³⁹ Coincidence played a part in this process. Wilson suggests that when Columbus first met with some of the caciques of Hispaniola, he accidentally fulfilled Taíno expectations regarding the acceptance and extension of hospitality: "Colón, through the luck of sitting down to eat when he did, kept the Taíno ritual order of chiefly greeting intact—eat first, then exchange gifts."⁴⁰

Trade relations often developed out of earlier gift exchanges, although they may have been more congruent. It is obvious to us now that Indians (like Africans) had clear ideas of value, desirability, and fair exchange, a fact that seems to have surprised Europeans, who perhaps took the seemingly uneven exchanges of earlier gift-giving and the redistributive practices of many native groups as indications that indigenous Americans had no notion of value or property. But Indians often proved to be demanding and calculating trading partners, echoing European experiences in West Africa. Regarding the English expedition to Roanoke Island in 1584, Timothy Silver comments that "to Barlowe and his companions it seemed as if the natives paid outrageous prices for English goods . . . Indian traders, however, thought the rate of barter more than fair. Animal skins were common items, . . . whereas knives, hatchets, and utensils were looked upon as exotic luxury goods."⁴¹ Commercial relations in which both parties participated voluntarily usually succeeded only if they grew out of already-existing indigenous forms of activity (or their close analogues) or if Europeans offered items so highly prized that people would be willing to undertake new

³⁶ Axtell, *Beyond 1492*, 66.

³⁷ Clendinnen, "Fierce and Unnatural Cruelty," 17. Much of Clendinnen's discussion focuses on the extent to which the Spaniards were unable to grasp the meaning and intention of Mexican initiatives and responses. Clendinnen notes that "there are . . . indications that hint at extensive native manipulations, guile being admired among Indians as much as it was among Spaniards, and Spanish dependence on Indian informants and translators was total," "Fierce and Unnatural Cruelty," p. 21.

³⁸ Axtell writes that "European hospitality was not much different from Indian, except that its spirit was more calculating," *Beyond 1492*, p. 64.

³⁹ Lockhart, *The Nahuas*, 445. He has elaborated this concept at greater length in "Some Nahuatl Concepts in Postconquest Guise," *History of European Ideas*, 6 (1985): 465–82.

⁴⁰ Wilson, *Hispaniola*, 65.

⁴¹ Timothy Silver, *A New Face on the Countryside: Indians, Colonists, and Slaves in South Atlantic Forests, 1500–1800* (Cambridge, 1990), 68. He adds that "coastal Carolina natives probably already knew something of the value of such items thanks to two European ships that had foundered off the treacherous Outer Banks some twenty-five years earlier." See also note 12 above.

economic activities. Where these conditions did not exist, trade broke down or did not begin at all.

Furthermore, the free operation of commerce presupposed the existence of products that were of interest to Europeans. Where commerce failed to yield such items, or yield them in sufficient quantity, Europeans might become producers rather than traders, and the very different requirements of production often entailed the imposition of coercive mechanisms. Columbus's failure to obtain much gold through barter in Hispaniola led in rapid succession to the attempted exaction of gold in tribute and finally to the establishment of mining operations staffed by Spaniards and African slaves and worked by enslaved or coerced Indian laborers. In places where free commerce did function—for example, in West Africa and North America—the participation of Europeans led to a rapid escalation in the scale of commercial activity, with a range of social, economic, and environmental consequences. Scholars have focused in particular on the impact and significance of the introduction of alcohol and firearms in parts of the Americas and in West Africa.⁴² Even if early trade relations in parts of the Americas were fairly balanced, Europeans set the terms of commerce, which in the long run favored them. Silver notes that, "although Indians continued to maintain some control over the ways in which goods were exchanged, their demand for liquor and other items gradually worked to undermine native independence. Indians had learned to use cloth, tools, and liquor, but not how to manufacture them."⁴³ Where Europeans and indigenous Americans experienced the most intense and extended contact, initial, fairly equal trade relations proved transitional, and coercive forms of exchange often took their place.

The formation of alliances also reflected the indigenous situations. Alliance, or apparent alliance, with European newcomers could be prestigious or signal a potential gain in power relative to competitors.⁴⁴ The political fragmentation characteristic of many parts of the Americas benefited the Spaniards, who, for example, allied with Tlaxcala against its bitter enemy Tenochtitlan and took advantage of the political instability of the Inca empire.⁴⁵ The pattern of alliances became increasingly complex as hostilities escalated, more or different groups of Europeans appeared on the scene, and the movement of populations (or certain sectors of them) accelerated, as occurred in the Caribbean, where by the seventeenth and eighteenth centuries conflicts and alliances involved not only the English, French, Dutch, Spanish, Caribs, and buccaneers (of several nationalities) but also maroons (escaped African slaves) and Black Caribs.⁴⁶

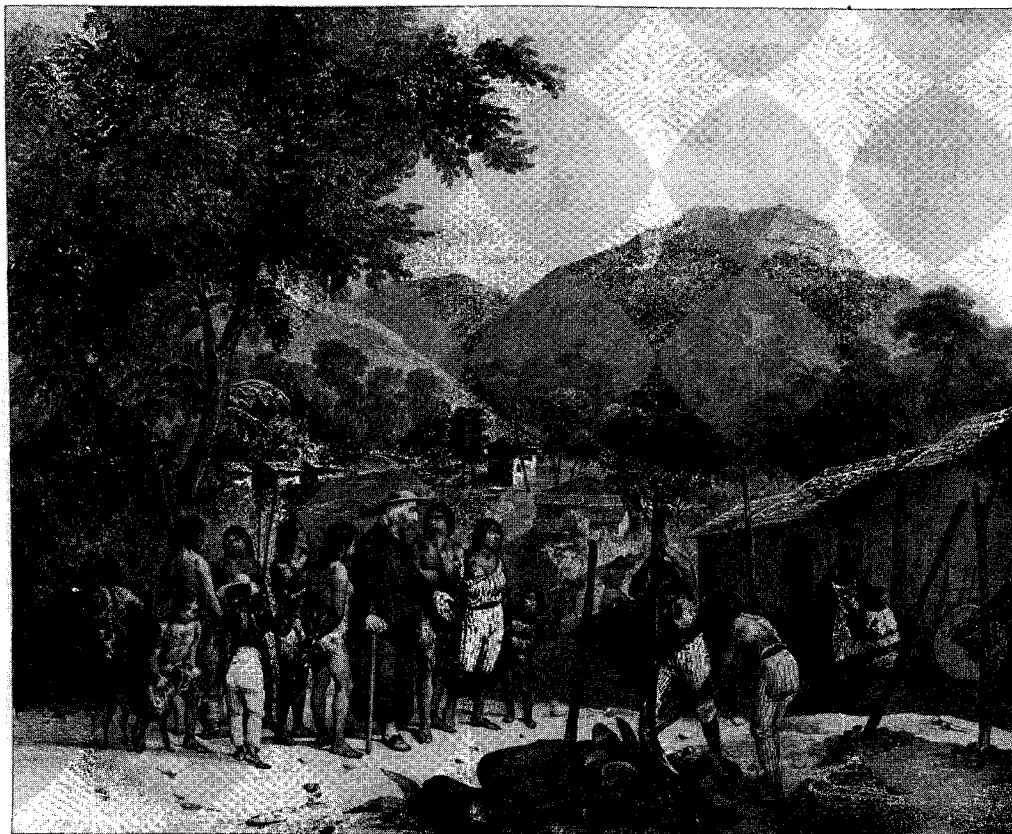
⁴² See Axtell, *Beyond 1492*, 142–43; for a somewhat different view, compare Gary B. Nash, *Red, White, and Black: The Peoples of Early America*, 2d edn. (Englewood Cliffs, N.J., 1982), 242–44; see also William B. Taylor, *Drinking, Homicide and Rebellion in Colonial Mexican Villages* (Stanford, Calif., 1979). On firearms in Africa, see Thornton, *Africa and Africans*, 113–25.

⁴³ Silver, *New Face*, 88, see also 192–93.

⁴⁴ Wilson writes that, in the days after Columbus landed on Hispaniola, "competition among local caciques for the explorers' attention was intense" and that "people of the entire northeast coast of the island tried to meet with the Europeans, have them come to their villages, and exchange food and presents," *Hispaniola*, pp. 66, 67.

⁴⁵ See Clendinnen, "Fierce and Unnatural Cruelty," on the Tlaxcalans; and Guilmartin, "Cutting Edge," on the state of the Inca empire.

⁴⁶ Boucher writes that by the mid-eighteenth century "at St. Vincent . . . an increasing French presence was largely at the invitation of Caribs, more and more the losers to Black Caribs in a fierce



"Aldea de la Tapuyos" [village of the Tapuyos], from Johann Moritz Rugendas, *Viagem pitoresca através do Brasil* (1835; Rio de Janeiro, 1972), courtesy of the Library of Congress.

THE PERIOD OF INITIAL CONTACT, for all its compelling if sometimes opaque drama (crucial aspects of events and responses will always elude us) was short-lived in the places where Europeans went and stayed. With continuing exposure, early relations evolved into something else. Early curiosity, awe, friendliness, fear, or hostility gave way to mutual influence and adaptation to new situations as Europeans, Africans, and Indians came together in the establishment of European-directed enterprises. Earlier events and contacts were by no means irrelevant to later developments, however, and probably in many ways set the tone, as well as creating the context, for subsequent interactions. John Guilmartin writes that "the way wars are fought frequently exercises a dominant influence over the ensuing peace. It would seem that the suddenness and totality of the Inca military defeat lessened the amount and scope of cultural transfer in other areas of human endeavor. Having learned to despise the Indians as armed foes, the conquistadors and their descendants were ill-disposed to respect them as subjects." His suggestion that the ease of the Spaniards' victory reinforced their notions of unques-

struggle to control the island. Island Caribs (now called 'Yellow' Caribs in contemporary documents) tried to prevent this outcome by alliances with the French, but without long-term success," *Cannibal Encounters*, p. 95.

tionable cultural superiority is important for understanding the post-conquest context in which Spanish-indigenous relations developed.⁴⁷

Axtell states that "in the face of new challenges from a people and a world they had never before known or even imagined, the various Indian groups worked mightily and often cleverly to maximize their political sovereignty, cultural autonomy, territorial integrity, power of self-identification, and physical mobility."⁴⁸ Although he refers mainly to the tribes of North America, his observation could apply equally to indigenous groups all over the Americas and, to a notable degree, to the Africans who lived there. While operating in many senses under far more severe constraints of movement and choice than did Indians under European rule, Africans nonetheless succeeded collectively in maintaining significant elements of their material and spiritual culture, influencing other groups with whom they had contact and the face of the land itself.⁴⁹

Maximum contact among all groups took place in and around the main centers of European activity—towns, estates, and mines.⁵⁰ The concentration of European political and religious institutions and population in those places, especially in towns and cities, meant that these settings contained structural and symbolic factors such as full-scale, stable European households, European-type trades and crafts, and the panoply of church and government that bolstered European cultural influence in language, religion, and technical skills. Nonetheless, it must be borne in mind that, where concentrations of population were largest, they were often mainly non-European in composition, meaning that, even in those places that most closely replicated the culture of the "Old World," Europeans were a minority surrounded by and dependent on the labor and productivity of others.⁵¹

⁴⁷ Guilmartin, "Cutting Edge," 62. Clendinnen also suggests an early "set" to post-conquest relations, although she is less explicit about its causes. She writes, "I am persuaded the terms of the relationship between the incoming and the indigenous peoples were set very early . . . In their first encounters with the peoples of Mexico the Spaniards had declared themselves profoundly impressed . . . Then something happened, a crucial break of sympathy," "Fierce and Unnatural Cruelty," p. 36.

⁴⁸ Axtell, *Beyond 1492*, 100. He continues that, in order to fend off the extension of European control, "natives resorted to five basic strategies, which were not always sequential or mutually exclusive: initially, they tried to *incorporate* the newcomers; when that failed, they tried at various times to *beat* them, to *join* them, to *copy* enough of their ways to beat them at their own game, and to *avoid* them altogether."

⁴⁹ See Thornton, *Africa and Africans*, Part 2: "Africans in the New World," esp. Chapter 8, "Transformations of African Culture in the Atlantic World"; and also Silver, *New Face*. For a somewhat different viewpoint on African cultural survival from that developed by Thornton, compare Sidney Mintz and Richard Price, *An Anthropological Approach to the Afro-American Past: A Caribbean Perspective* (Philadelphia, 1976).

⁵⁰ See James Lockhart, "Trunk Lines and Feeder Lines: The Spanish Reaction to American Resources," in Andrien and Adorno, *Transatlantic Encounters*. The existence of urban nuclei controlling (and living off) a countryside from which they are at least to some extent culturally and institutionally distinct was a pattern characteristic of situations of conquest and colonization from ancient times and certainly was familiar to Europeans in the medieval period. Fernández-Armesto notes, for example, that "the thoroughly Catalan appearance of Majorca in the next [14th] century was a result of Catalan predominance in the main centers of population and commerce," *Before Columbus*, p. 23.

⁵¹ Mexico City in 1550, for example, probably had around 8,000 European, 8,000 African, and about 75,000 Indian inhabitants. Cities and towns in British North America were the main exceptions. Compare also the statement of Fernández-Armesto that "the colonization of Andalusia [after the reconquest] evidently depended on non-Castilian settlers . . . At first glance, the most surprising alien community to be welcomed among the settlers was that drawn from among the Moors themselves," *Before Columbus*, p. 66.

On the margins of these centers of European society, the forms and consequences of contact could be quite different. The way of life that evolved among maroons in many parts of the Americas owed a great deal to indigenous practices,⁵² and the French *coureurs des bois* in Canada or the *coureurs des îles* married indigenous women and lived much as their native hosts and trading partners did. Interactions on the margins were not always harmonious, doubtless partly a result of indirect pressure exerted by European activity and intrusion elsewhere. From the sixteenth century, Caribs captured African slaves from Spanish territories and kept them for their own use, and they acted as middlemen in the European trade in runaway slaves; Black Caribs and "Island" Caribs vied for control in the Lesser Antilles.⁵³ Still, the existence in close proximity of many small, competing communities may have contributed to the survival of newly forming marginal groups.⁵⁴ Even at the very core of the "New World" that was being forged from disparate cultures, there existed worlds that were separate and distinct—at least partially autonomous, impervious to outside cultural influences to a notable degree, and with their main concerns internally and not externally determined and directed. This was as true of European communities in the Americas as it was of indigenous or African ones.⁵⁵

Not surprisingly, it was outside the towns and off what Lockhart calls the "trunk lines" of European activity that religious conversion probably made the least progress (even though the countryside was the focus of much "missionary" activity) and overt resistance to European rule was the most vigorous. The Andean highlands, where indigenous populations were dense and Spanish settlement was far more limited than in central Mexico, were the scene of frequent rebellions in the colonial period; the same was true also for much of Yucatan and Central America. As Weber notes, "violent eruptions that Spaniards characterized as rebellions but which Indians probably saw as armed struggles for liberation" were frequent in Florida and New Mexico in the late sixteenth and seventeenth centuries, perhaps more frequent than official sources revealed and often

⁵² Price notes that "relations between Saramakas and Amerindians ran the gamut from limited friendship and solidarity to bitter enmity . . . Much of Saramaka material culture and horticultural technique . . . was learned from Indians. But . . . occasional cases of solidarity and cohabitation should not mask the more generalized fear that Saramakas had of those Indians (most often Caribs) who served the government as the most effective of jungle scouts and bounty hunters against them," *Alabi's World*, p. 403, n. 68.

⁵³ See Boucher, *Cannibal Encounters*, 49.

⁵⁴ Thornton suggests that "even if Native Americans did not harbor slaves, their very presence might allow runaways more freedom to develop their own communities" and that "the politics of Native Americans, with their numerous small political units often hostile to each other, favored the formation of separate runaway communities," *Africa and Africans*, pp. 286, 291. His discussion (282–92) touches on many issues of relations between Indians and maroons, including the enlisting of Indians to return runaway slaves (and the use of maroons to return other runaways).

⁵⁵ Lockhart remarks of his subject that "I wished to show . . . that such groups long continued to constitute an immensely complex, partly autonomous sector that must be studied on its own terms," *The Nahuas*, p. 417. See also Breen, who emphasizes the conservatism of early Americans. He writes that "sometimes what appears to have been a radical cultural reorientation turns out upon closer examination to have been a shrewd compromise, a superficial adjustment masking the survival of a deeper, often symbolic cultural core," "Creative Adaptations," p. 215.

resulting in the deaths of missionaries. He suggests that the Spaniards themselves "understood the Pueblo Rebellion as a rejection of Christianity."⁵⁶

Where conversion efforts did meet with some success, the religions that emerged over time among Christianized Indians and Africans in the Americas resulted from a complex process in which elements of Christian doctrine, ritual, and popular belief were absorbed into older religious systems at those points of greatest congruence between old and new. There were close parallels, for example, between Spanish popular beliefs regarding the saints and the Nahuas' views of their gods.⁵⁷ Thornton remarks that "both Christianity and African religions were constructed in the same way, through the philosophical interpretation of revelations. Africans, however, unlike Christians, did not use these religious interpretations . . . to create an orthodoxy."⁵⁸

Arguably, many native American as well as African religions were less rigidly orthodox and therefore in a sense more flexible or tolerant than Christianity, facilitating conversion of a sort. What often took place was not so much full conversion as the incorporation of some parts of Christian practice into existing belief systems.⁵⁹ Thornton asserts that "African Christianity . . . continued to recognize many other revelations as valid, and moreover, it never fully accepted certain points of Catholic doctrine, especially those that reinforced the power of the priesthood (the primacy of discontinuous revelations such as the Bible)."⁶⁰ The term "syncretism," frequently used in reference to the religious systems that developed in the Americas after contact, may not adequately reflect the process that actually took place. By analyzing the translation of catechisms for religious instruction into Nahuatl, Louise Burkhart demonstrates how the very effort to find linguistic equivalents for terms expressing key elements of Christian doctrine (sin, immorality, the Virgin Mary) subtly or significantly changed their meaning.

⁵⁶ See Weber, *Spanish Frontier*, 133–45; also see Nancy Farriss, *Maya Society under Colonial Rule* (Princeton, N.J., 1984); and Grant D. Jones, *Maya Resistance to Spanish Rule: Time and History on a Colonial Frontier* (Albuquerque, N.Mex., 1989). See Castro-Klarén, "Dancing and the Sacred in the Andes"; Sabine MacCormack, "Demons, Imagination, and the Incas," in Greenblatt, *New World Encounters*; and Rolena Adorno, "Images of Indios Ladinos in Early Colonial Peru," in Andrien and Adorno, *Transatlantic Encounters*, on aspects of Andean religion and continuities in the post-conquest period.

⁵⁷ "In Spain, the corporate aspects of local religion were expressed through images of saints with specialized supernatural powers, each image having its own attributes and being associated with a particular region, town, social group, or subdistrict. Among the Nahuas a pantheon of specialized gods behaved in precisely the same manner." Lockhart, *The Nahuas*, 243. The diversity of Christian belief and practice among Europeans doubtless contributed to this process. See Merchant, *Ecological Revolutions*, 103–05, on English popular religion, which "offered a counterpoint to Christian doctrine and congregational life," p. 103. MacCormack, *Religion in the Andes*, 35–37, discusses Christian heterodoxy in Spain and notes that "non-Christian religious observances of Islamic and Jewish, popular and pagan origin endured in the peninsula long past the time of Columbus," p. 35.

⁵⁸ Thornton, *Africa and Africans*, 246; see his chapter "Religions in the Atlantic World" for a full comparison of Christian and African religious systems.

⁵⁹ Weber, however, suggests that the Pueblo Indians kept the two religions separate "well into the twentieth century," *Spanish Frontier*, p. 118.

⁶⁰ Thornton, *Africa and Africans*, 254. Yet MacCormack, *Religion in the Andes*, 17, notes that "in Spain, as elsewhere in Christian Europe, visionary and prophetic experience had not come to an end with biblical times," so perhaps the distinction between continuous and discontinuous revelations is not as sharp as Thornton implies.

The result, she argues, was not a blend of the two religions but rather a Nahua belief system with a Christian overlay.⁶¹

Under the circumstances of conquest or enslavement, the question of conversion was not, of course, strictly a religious one. Embracing Christianity—or perhaps more specifically its agents and trappings—could reflect a choice that was as much political as spiritual. Axtell suggests that “the majority of Indian neophytes turned to the invaders’ cultures and religions for empowerment, knowledge, and skills with which to sustain native identities and values in other guises.” He points out that education appeared to offer the means to deal effectively with the representatives and demands of the colonial world.⁶² Richard Price discusses the possible attraction for the Saramakas of associating with Moravian missionaries, who seemed to hold out “a potential entree into the mysterious and hitherto forbidden world of reading and writing.”⁶³ However, conversion or the ability to read and write did not necessarily redress the imbalances created by the imposition of European rule and institutions.

Among the most fascinating if elusive products of the process by which Indians and Africans adopted and adapted to European culture were individuals who functioned as intermediaries between cultures and societies that to an impressive extent continued to function largely self-referentially. Frequently, these individuals occupied traditional positions of leadership and hence were of particular interest to Europeans, who cultivated their support and used their authority to further European objectives. The central figure in Price’s study, Alabi, attained the highest position of authority among the Saramakas, notwithstanding his apparently wholehearted embrace of Christianity. Enrique, the Taíno cacique who led a rebellion that lasted from 1519 to 1532 on Hispaniola, had been educated by the Franciscans. At one point in the negotiations for peace with Spanish authorities, he asked for clothing not only for himself and his family but also for his “imágenes” (figures) and for a priest to come baptize children born during the rebellion.⁶⁴

Not all intermediary figures arose from the ranks of traditional leadership. Thomas “Indian” Warner, who served in the seventeenth century as deputy-

⁶¹ In her conclusion to *The Slippery Earth*, Burkhart writes that “the logical structure of the universe remained Nahua; Christian elements transformed to fit this logic—to express symbolic relationships that made sense in a Nahua context—could be accepted because they no longer challenged the very nature of reality” and that “Christian teaching was effective only to the extent that it was compatible (or appeared in translation to be compatible) with preexisting belief and practice. Changes in the pantheon involved much more reworking than replacement,” pp. 188, 190. MacCormack emphasizes the importance of recognizing the evolution of belief systems over time: “while Spanish understanding of the Andes changed, so did Andean religious thought and observance. Moreover, in due course, Andeans themselves addressed the invaders on the subject of religion,” *Religion in the Andes*, 6.

⁶² James Axtell, “Native Reactions to Invasion,” in *Beyond 1492*, 116, 118.

⁶³ Price, *Alabi’s World*, 67–68; he continues that “Saramakas then as now were caught in this terrible bind, knowing that literacy was a password to an understanding of the outside world and the key to being able to manipulate it, but also knowing that its acquisition entailed what was, for them, a truly Faustian bargain, the willingness to sell their souls.” Price also suggests that the desire to have a relationship with the missionaries “seems to have had less to do with some notion of gaining prestige than it did with gaining an avenue to the white power structure in the city.” See also p. 95.

⁶⁴ Thornton, *Africa and Africans*, 284–85, mentions the participation of runaway African slaves in this rebellion. On the negotiations with Enrique, see Roberto Marte, ed., *Santo Domingo en los manuscritos de Juan Bautista Muñoz* (Santo Domingo, 1981), 296, 346, 364–65.

governor of Dominica, was the illegitimate son of an early colonial governor. He apparently "refused to live as a Christian," and some of the British, especially his legitimate half-brother Philip Warner, suspected his loyalties.⁶⁵ Rolena Adorno's discussion of the experience of *indios ladinos* in Peru reveals the cultural complexity and psychological tension experienced by individuals who functioned within a colonialist framework that embraced dissonant and conflicting elements.⁶⁶ Yet it may have been not only the strain of those contradictions in themselves but also the forceful imposition of European colonial demands that impelled many such intermediaries to become active participants in movements of resistance and rebellion.⁶⁷

TRANSFORMATION OF THE LAND and of the relationship between people and their environment manifests itself in the historical record, which reflects patterns and density of settlement, land titles, transactions, legacies, and shifts in productive trends, and in the biological record. The study of ecological change is potentially one of the most revealing approaches to understanding the effect of contact between societies with diverse cultures.⁶⁸ Recent scholarship that treats aspects of environmental change not only draws on more than one discipline but is regionally specific and, like other kinds of contact studies, takes into account all the relevant actors. The result, in works such as Silver's, is a detailed, dynamic, and at times almost kaleidoscopic picture of ecological evolution in which Indians, Europeans, and Africans all participated. Their interactions with one another, with indigenous and imported plants and animals, and with the land itself, formed the context of the colonial world.

The relationship of people to the land from which they draw subsistence and wealth defines a society and its culture to varying degrees. The peoples of the Americas at the time of contact had strong notions of territoriality (in the sense of a group's identification with a particular area, whether it was where a tribe hunted or where the members of a corporate unit farmed). This sense of territorial

⁶⁵ See Boucher, *Cannibal Encounters*, 67.

⁶⁶ Adorno suggests that "writing was an institution of colonization, the *indio ladino* quickly understood, and to enter into its use ultimately became an anticolonialist stance," "Images of Indios Ladinos," p. 257. *Indios ladinos*, used as interpreters in Spanish-American society, were not, as Adorno notes, a coherent or homogeneous group, and as individuals were often obscure. She devotes much of her discussion to the chronicler Guaman Poma de Ayala.

⁶⁷ Adorno writes that "later generations of *indios ladinos* who worked as *lenguas* or interpreters seem to have been recruited voluntarily, not always by force . . . At the same time legislation against coercion and effective enslavement of interpreters indicates that such problems continued to exist throughout most of the sixteenth century. The rebellion of *indios ladinos* via their participation in revivalist movements thirty years after the Spanish conquest of Peru suggests that coercion might have been a significant aspect of their early Hispanization," "Images of Indios Ladinos," p. 234.

⁶⁸ Its study virtually requires an interdisciplinary approach, since the methods and data of history, historical and cultural geography, meteorology, biology, and geology are all relevant. Merchant, for example, brings to bear her background in environmental history and philosophy in her study *Ecological Revolutions*. See also Silver, *New Face*. Alfred W. Crosby, *The Columbian Exchange*, and, more recently, *Ecological Imperialism: The Biological Expansion of Europe, 900-1900* (New York, 1986), stimulated new interest in questions related to ecological change in the Americas (and elsewhere in the world) resulting from contact with Europe. See also William Cronon, Jr., *Changes in the Land: Indians, Colonists, the Ecology of New England* (New York, 1983).

identity ranged from concepts of landholding that appear to come very close to those of some Europeans⁶⁹ to more flexible views of territorial rights that could be specific without necessarily being exclusive.⁷⁰ Notwithstanding such variation, we might suggest that, for native American cultures, the perception of and relationship to the land and its products played a more central role in their spiritual life and self-conception than they did for Europeans.⁷¹ Although Europeans also held beliefs and superstitions connected with the land and agricultural practices and recognized the sacredness or special power of certain places in the countryside,⁷² when they went to the Americas, they settled on and laid claim to lands that lacked any special meaning for them and that they intended to exploit for subsistence and profit.⁷³

Whatever the Europeans' intentions in America, their relationship with the land inevitably reflected and incorporated indigenous and African techniques and crops. In eastern North America, the English took advantage of the "availability of Indian fields," as well as the Indians' techniques for clearing fields, and shifted to growing indigenous plants—corn, above all, but also beans and squash. An indigenous herb, tobacco, brought prosperity to English settlers in the Chesapeake.⁷⁴ In some places, European crops would not thrive. In Brazil, for example, wheat did not replace cassava as the principal staple. In other cases, Europeans left production of key items—cochineal and cacao in Mesoamerica, for instance—entirely in native hands. The experience and skills brought by African slaves were crucial in such activities as clearing fields and swamps, rice cultivation, production of naval stores, and cattle ranching.⁷⁵ Thus, whether done voluntarily

⁶⁹ Lockhart refers to "European land tenure, where all private holdings entail public responsibilities and under certain conditions can be repossessed by public agencies. Nevertheless, the dominant relationship between public and private in the European tradition is that land is either one or the other, while the dominant relationship in the Nahua tradition was that it was both at the same time," *The Nahuas*, p. 162.

⁷⁰ In this kind of situation, the conceptual differences regarding the relationship between people and territory that existed between Europeans and Indians were considerable. Merchant writes that, "to the Indian, white pressure to cede territory meant extending hunting and fishing privileges on tribal homelands to the newcomers. To the English, however, it meant release of all tribal rights," *Ecological Revolutions*, p. 57.

⁷¹ See MacCormack, *Religion in the Andes*, 95–98, on the relationship between Andeans and the land. "Throughout the Andes, people thought their ancestors had sprung from the land itself . . . The relationships Andeans perceived between life and death, and between humanity and the natural environment were profoundly different from their Spanish and Christian equivalents," p. 97. See also p. 180.

⁷² See Merchant, *Ecological Revolutions*, chap. 4; MacCormack, *Religion in the Andes*, 36.

⁷³ Scholars frequently cite the tendency of early Europeans in the Americas to rename places as the first act of establishing possession; Axtell comments that "the first Europeans signaled their imperial intentions by naming or renaming everything in sight," *Beyond 1492*, p. 59. He suggests that the intention might have been primarily to deflect European competitors. In many cases, indigenous names proved tenacious. Under the Spaniards, for example, the name Tenochtitlan gradually gave way to Mexico, but the name Mexico itself comes from the people who built and inhabited the city before the Spanish arrived there. One is left with the impression that the Europeans often found the new (European) names awkward, although their early misunderstanding of indigenous terms produced such peculiar and nonsensical anomalies as Peru and Yucatan.

⁷⁴ Silver, *New Face*, 105–06, 141–42.

⁷⁵ See Silver, *New Face*, 106–08, 124, 173–74. He writes, "Like agriculture and the forest industries, the southern livestock trade could not have flourished without skilled slave labor. On the large plantations along the Chesapeake, planters relied on slaves to see to the daily needs of their herds. In the backcountry, blacks probably played even more prominent roles," pp. 173–74.

or not, the exploitation of land and its productivity was a collaborative effort that to some degree altered European perceptions and techniques, even as it modified the context in which everyone functioned.⁷⁶

Despite the development of variegated forms of production that drew on the experience and skills of all the participants, Europeans largely determined the direction of ecological change (although occasionally with unintended results). In early periods of settlement, there may not have been much direct competition for land, especially since indigenous populations declined, sometimes precipitously, as a result of imported diseases.⁷⁷ But European and native ways of exploiting the land differed considerably, and the former tended to expand at the expense of the latter. European-introduced livestock invaded indigenous fields or, conversely, European fencing fragmented what had been open space.⁷⁸

Exploiting the land involved not only the form in which it was used (intensively, extensively) and for what purposes (choice of crops, stock raising versus cultivation) but also the uses of labor. Here, again, European objectives fostered changes in social and environmental relationships that, even if initially limited principally to the people and areas directly under colonial control, eventually had widespread and long-term repercussions. Europeans expected native men to perform agricultural labor, contrary to the custom in most indigenous societies of women being primarily responsible for cultivation or both sexes participating.⁷⁹ Demands for labor in places such as Hispaniola and later on the mainland, where men were removed from their homes and fields to work in mines, construction projects, or other European-directed undertakings, could destroy indigenous agriculture and

⁷⁶ See Merchant, *Ecological Revolutions*, 144–45, who contends that in New England “the colonial ecological revolution had transformed native American ecology and consciousness while also producing a synthesis of European and Indian methods of production . . . Puritan attitudes toward nature softened as ‘wilderness’ was ‘civilized,’ generating an organic cosmology and participatory consciousness that informed rural farming practices.” In the eighteenth century, however, “a movement toward agricultural improvement coupled with a mechanical philosophy and an analytic consciousness was beginning to take shape.” Merchant finds that New England—and, by implication, much of North America—experienced two major ecological and social revolutions, which she refers to as colonial and capitalist, after the arrival of Europeans.

⁷⁷ See Lockhart, *The Nahuas*, 164, on the impact of demographic decline on landholding among the Nahuas.

⁷⁸ Merchant writes that “Indians’ shifting agriculture left them particularly vulnerable to colonists practicing settled agriculture” and that “Indians needed thousands of upland and forest acres for hunting . . . English cattle and pigs, grazing in forest and marshes, altered the habitats that supplied Indians with animals for clothing and food. The English demolished beaver dams to create fertile fields and drained marshes that had supplied rushes for baskets and mats,” *Ecological Revolutions*, pp. 86–87. On fencing, see p. 63.

⁷⁹ “To colonists, ‘civilizing’ the Indians meant converting their female-dominated shifting horticultural production into male-dominated settler farming. Despite the power of women in production, colonial fathers dealt only with Indian males.” Merchant, *Ecological Revolutions*, 92. Europeans frequently failed to recognize the roles played by women and the function of gender in indigenous societies. It is generally very difficult to discuss women in early episodes and periods of contact, certainly not because they were absent but rather because male European observers often did not describe (or inquire about) things that to them seemed of little interest or importance. MacCormack comments on the cult of the Moon, Pacsamama, that “this latter cult was in the care of women, which is why Spanish historians, all of them men, learned so little about it,” *Religion in the Andes*, p. 115. Her remark implies not only men’s lack of recognition of a special female sphere of religion but also women’s indisposition to reveal ritual practices or special knowledge to men or outsiders. See also Montrose, “Work of Gender,” 201–03, on Raleigh’s discussion of Amazons, who were perhaps considered a more acceptable topic (as a product of classically inspired imagination) for description or speculation than actual indigenous women.

family life.⁸⁰ Even if ultimately unsuccessful, policies such as the *congregación* (intended to concentrate diminishing or scattered populations in fewer settlements, in part to facilitate the exaction of labor or tribute) undermined and disrupted community life, as did the out-migration of adult males who sought to escape labor and tribute demands.⁸¹ Europeans introduced slavery into societies in which it was previously unknown, although probably only in the Caribbean and circum-Caribbean on a scale that proved devastating, where whole areas (such as the Bahamas) were virtually depopulated as a result. Africans who were brought to the Americas in the slave trade experienced even greater disruption of family and community life than did indigenous Americans. Their social and ethnic bonds came under tremendous strain since, as involuntary migrants, they were not free to choose their destinations with the expectation of joining others from their homelands, although they did cluster and reconstitute traditional ties wherever possible.⁸² Thus the labor systems introduced by Europeans affected social relations and altered people's relationship to the land, removing them from traditional lands or changing the way in which lands were used.⁸³ People had been using and modifying the landscape of the Americas for millennia, but the European introduction of new plants and animals and ways of using human labor and the land initiated an epoch of radical change.

WE HAVE NOTED THAT THE OUTCOME of many forms of contact tended to favor the objectives of Europeans in the long run; by way of conclusion, we wish briefly to suggest why that was so. Comparisons of European, African, and indigenous American cultures and societies of the period at first glance do not necessarily reveal glaring inequalities that would seem to ensure the easy success of one at the expense of the other. In all these regions, cultures were vital, varied, and complex; in some instances, local and regional economies were highly productive, although in others the subsistence base was marginal and precarious. All had social systems with some degree of flexibility that allowed them to evolve, expand, or fragment. This fluidity, more characteristic of some cultures than others, permitted the mutual influences and adaptations that took place after contact among Europeans, Africans, and Indians discussed here.

Thornton argues that Europeans did not enjoy any significant technological,

⁸⁰ See Wilson, *Hispaniola*, 96.

⁸¹ See Kenneth J. Andrien, "Spaniards, Andeans, and the Early Colonial State in Peru," in Andrien and Adorno, *Transatlantic Encounters*, esp. 125–27, 134–36.

⁸² See Thornton, *Africa and Africans*, 192–205, on the homogeneity of some slave groups and clustering in the Americas; he concludes that "wherever more than a few slaves from the same nation were concentrated, in towns or on estates, the possibility of cultural transfer was possible . . . The slave trade and subsequent transfer to New World plantations was not . . . quite as randomizing a process as posited by those who argue that Africans had to start from scratch culturally upon their arrival in the New World," p. 204.

⁸³ Merchant, for example, suggests that "the horticulture of Indian women began to reconstruct the environment, but their production system mimicked natural patterns . . . The colonial ecological revolution in the New England homeland was externally caused and resulted in the erosion of Indian modes relating to nature. Local ecology on which Indian gather-hunting and agricultural production depended was disrupted by plant and animal introductions and by fur and timber extractions," *Ecological Revolutions*, pp. 108–09.

economic, or military advantages over Africans (and in fact suffered from real disadvantages in the African disease environment). He points out that Europeans exercised little control *in Africa*; they controlled Africans in the Americas because they removed them—with the cooperation of African states and merchants—from their homelands. Technological disparities doubtless played an important role in European success in the Americas: lighter, faster, and more powerful weapons facilitated military conquest,⁸⁴ and the possession (and sources) of desirable tools and weapons contributed to a growing imbalance in commercial relations.

Epidemic disease and resulting demographic change, which affected the Americas but not Africa, created a situation in which indigenous societies underwent a variety of internal changes resulting from extremely high mortality,⁸⁵ while at the same time the external social and economic pressures exerted by the Europeans were on the increase.⁸⁶ Warfare, often on an unprecedented scale, and subsistence crises occasioned by disruptions in traditional agriculture or hunting, compounded the dislocating effects of disease.⁸⁷

We suggest, however, that while technological and epidemiological factors played a crucial role in Europeans' imposition of their institutions and economic systems, other factors inherent in the social and political systems of Europe, Africa, and America facilitated that success as well. Not only were their objectives clearly articulated, but the Europeans arrived in the Americas prepared to take whatever measures were necessary to achieve them, which translated into a crucial advantage over the people of the relatively small political and social units they confronted. Guilmartin, for example, emphasizes the cohesion of Spanish forces in combat; "under pressure they stuck together with a seemingly instinctive

⁸⁴ Guilmartin states that "the single biggest tactical advantage accruing to the conquistadors was that Spanish swords, pikes, and lances could strike far more quickly and lethally than Andean copper- or stone-tipped spears, clubs, and axes. This was due partly to the swiftness of the lighter Spanish weapons and partly to the ineffectiveness of crushing weapons against armor and horsehide . . . Horses gave the Spanish crucial advantage in striking power, shock effect, and speed," "Cutting Edge," p. 53.

⁸⁵ See Axtell, *Beyond 1492*, 105–07, for a discussion of some of the strategies adopted by North American Indians in face of demographic losses. See also Silver, *New Face*, 74–83, on the impact of disease. He notes that "east of the mountains, where contact with Europeans was more sustained and the Indian population decline more severe, Old World diseases took an especially heavy toll among elderly natives . . . By killing older Indians in disproportionate numbers, the new diseases gradually eroded the natives' cultural memory and restricted the dissemination of traditional lore," p. 88. Everywhere in the Americas, demographic loss differed according to degree of contact with Europeans; Silver points out that the relatively isolated Cherokees possibly had recovered their pre-contact population numbers by the late seventeenth century, p. 82.

⁸⁶ Lockhart states that "the increasing number of Hispanics and decreasing number of Indians was clearly much of the reason for the progressive increase in the amount of contact between the two populations . . . The impact of a few thousand Spaniards among millions of Indians was inevitably heightened as the numbers of the Nahuas fell to half, then a quarter, then less than that," *The Nahuas*, p. 433. Merchant writes that "biological reproduction and social reproduction were altered by Indian depopulation, the growth of colonial population, and an introduced system of property ownership that put increasing pressure on Indians to cede more land," *Ecological Revolutions*, p. 90.

⁸⁷ See Clendinnen, "Fierce and Unnatural Cruelty," 37–38, on the siege of Tenochtitlan; Merchant, *Ecological Revolutions*, 90, on the effects of warfare in seventeenth-century New England; Wilson, *Hispaniola*, 89–90, 94–96, 135–36, on early conflict in Hispaniola (which took the lives of a number of caciques, the traditional leaders) and evidence of famine.

solidarity rarely matched in the history of war."⁸⁸ Europeans' discourse tends to reflect their clarity of purpose and the ways in which the New World and its people were reduced to categories appropriate for domination and exploitation.⁸⁹ While indigenous Americans were in no sense unaccustomed to conquest and warfare, they usually conceived of and undertook more circumscribed conflicts than did Europeans.

Even though Europeans knew what they sought (profits, access to resources), they lacked sufficient manpower to achieve their ends. Their great luck, one might say, was that they encountered societies with well-developed mechanisms for amassing and allocating labor—slave labor in Africa, rotating public labor duty and other kinds of labor service in the most densely populated parts of the Americas such as Mesoamerica and Peru⁹⁰—that they could exploit. Where they did not find such systems, as in eastern North America, the use of indigenous labor was minimal, and Europeans turned to their own working classes and to Africans for labor. They were able to exploit these labor systems not because of great disparities between themselves and these societies but because basic similarities of economic and political structure made it relatively easy to participate in commerce with African states that furnished large numbers of slaves or to dominate indigenous sociopolitical structures and hence utilize indigenous labor. Thus we might argue that it was not the superiority of European cultures but rather the fortuitous fit of European socioeconomic systems with African and American ones that, combined with Europeans' powerful ambitions, ensured their domination.⁹¹

⁸⁸ Guilmartin, "Cutting Edge," 55. See Thornton, *Africa and Africans*, 91, 103–05, on the nature and small size of West African states. On the European "drive for mastery," see Jack P. Greene, *Imperatives, Behaviors and Identities: Essays in Early American Cultural History* (Charlottesville, Va., 1992), 11–12. Wills reaches similar conclusions regarding European successes in Asia: "The Europeans had a technological edge at certain times and in certain ways, but it was not insuperable. Far more important were the organizational cohesion and staying power of their state and corporate organizations, their effective interactions with Asians, their exploitation of Asians' talents and practices," "Maritime Asia," p. 105.

⁸⁹ See Montrose, "Work of Gender," 195, on the feminization of the "new found land" in Raleigh's discourse; Merchant notes that "the association of Indians with animals also helped to legitimate the extermination of the red race through warfare," *Ecological Revolutions*, p. 65. Intellectual discussion of the nature of the Americas and Americans may fairly be considered to form a part of the context in which conquest and colonization took place rather than in any real sense providing the impetus for those undertakings or even determining their nature or direction.

⁹⁰ See John V. Murra, "'Nos Hazen Mucha Ventaja': The Early European Perception of Andean Achievement," in Andrien and Adorno, *Transatlantic Encounters*, 77, on the pre-contact organization of Andean labor.

⁹¹ At the same time, in many spheres of social and political organization and spiritual life, that fit was missing. The imperviousness of Europeans to cultural models that differed markedly from their own allowed them to use what was familiar and ignore what was not. Lockhart notes that "although Spaniards thus profoundly misunderstood the altepetl [Mesoamerican ethnic state], there was little on the surface to tell them that they were wrong, and in time their conception and their terminology were to have important effects on the Nahuas themselves," *The Nahuas*, p. 20. We might agree with Wilson, *Hispaniola*, 128, in concluding that, "in the set of interactions we can observe between [the cacique] Behechchio and Bartolomé [Colón], and throughout the conquest history of the New World, there is a remarkable degree of real conceptual understanding and commonality of purpose," while still recognizing the profound and enduring separateness of the cultures that formed the new societies of the Americas and the misunderstandings that existed between them.

Featured Reviews

LYNN HUNT, editor. *The Invention of Pornography: Obscenity and the Origins of Modernity, 1500–1800*. New York: Zone; distributed by Massachusetts Institute of Technology Press, Cambridge. 1993. Pp. 411. \$26.95.

I have been looking at a lot of pornography lately (strictly for research purposes), and come to the unsurprising conclusion that it's amazing what turns people on: everything from *Amputee Times* to *Wanda Whips Wall Street*. Academic books about pornography also run the gamut. The best ones are those like Linda Williams's *Hard Core* (1989) that give a real sense of their subject matter, although she did not include pictures, so the reader presumably is supposed to rent videos of "The Devil in Miss Jones" and "Behind the Green Door" and watch them between chapters. This collection of essays edited by Lynn Hunt is not as important or original, but it is an intelligent analysis of the early years of the phenomenon of pornography.

Although explicit depictions and descriptions of sex organs and activities have existed in most, probably all, times and places, Hunt argues that "pornography as a legal and artistic category seems to be an especially Western idea with a specific chronology and geography" (p. 10). This is probably true, but her argument would be stronger if she had included at least one essay on erotica in other cultures. Sexually explicit novels and pictures existed in China at least as early as the seventeenth century; an analysis of, say, *Rou putuan* ("The Carnal Prayer Mat") would have provided evidence for a cross-cultural comparison. Li Yu's account of how the After Midnight Scholar enlarges his member by means of an operation involving a dog's penis certainly takes the prize for a weird solution to a common concern: "You mean to tell me *everybody's* penis is bigger than mine?" (Li Yu, *The Carnal Prayer Mat*, Patrick Hanan, trans. [1990], p. 99).

Hunt and her co-authors do demonstrate, however, that pornography has a history, and a longer and more distinguished one than might be assumed. The word "pornography" only entered the English language in the mid-nineteenth century. (It appeared in French a century earlier.) Other key terms referring to sexual phenomena have a similar history:

"fetishism," for example, was first used in the psychosexual (as opposed to a religious-anthropological) sense in about 1887, roughly contemporaneous with the invention of terms such as "homosexuality," "sadism," and "masochism." Recent research in the history of sexuality has shed much light on these nineteenth-century developments, along with related matters like the development of *scientia sexualis* and the appearance of modern obscenity laws. Hunt and her co-authors, in contrast, focus on the period 1500–1800, the formative centuries of pornography as a modern concept. All students of sexuality owe them a debt of gratitude for their analysis of this early history, which has hitherto been relatively neglected.

Not just a product of the "Secret Museum" mentality of the Victorians, pornography is here shown to be related to the Renaissance, the scientific revolution, the Enlightenment, and the French Revolution. Pornographers also have a distinguished family tree, being related to heretics, free-thinkers, and libertines. It was only in the mid-eighteenth century, we are told, that pornography became "an aim in itself," rather than being an "adjunct to other forms of criticism of Church and State" (p. 16). Or should we say that pornography was born and came to maturity around 1660? (Dildos and condoms were for sale then.) Or did pornography first emerge in the sixteenth century along with print culture (p. 30)? There is some debate about specifics, but it is clear that premodern pornography was grounded in social issues that went far beyond sexual arousal. When sixteenth-century Italian pornographers wrote about the battle among the Pricks, Cunts, Balls, and Asses, they were composing political allegories, as well as aids to masturbation (p. 91).

Whether modern porn is still subversive or revolutionary is left an open question. The feminist/censorship aspects of today's great porn debate play little role in this volume, but the historical perspective provided here makes it harder to demonize contemporary porn. If you think of pornography in terms of

literature (*Fanny Hill* or Diderot's *Les Bijoux indiscrets* or, indeed, any novel that incites desire), or if pornography is conceptualized as an oppositional genre that revolts against "conventional morality" and reveals its "hypocrisy" by using the "language of transgression," then it sounds pretty good (p. 37). Even the nasty sexist stuff can be understood in terms of making explicit the nature of gender relations and providing an implicit "critique" (p. 40).

The only problem is, it all seems pretty remote from the lived experience of actually *getting off* on dirty books and pictures. It is entirely valid to focus on the social and political aspects of pornography; but most of the essays have the effect of making pornography seem surprisingly tame. Of course, pornography itself can be boring, maybe because it is so stereotyped: "Ma chère Octavie, mon Amour, embrassé moi étroitement, & reçois, ah, ah, ah, je n'en puis plus, je décharge ah, ah, ah, je meurs de plaisirs" (pp. 170–71). But it is precisely the stereotyped quality of pornography that requires some kind of analysis of the masturbator's latent fantasies.

Lucienne Frappier-Mazur does address this issue in her excellent essay on "Truth and the Obscene Word in Eighteenth-Century Pornography." She is one of the few authors to cite modern psychoanalytic studies, such as Joyce McDougall's "The Anonymous Spectator," the first chapter of McDougall's *Plea for a Measure of Abnormality* (1980). Paula Findlen's intelligent essay on "Humanism, Politics, and Pornography in Renaissance Italy" is also exceptional in giving a sense of how pornography was experienced. She also uses visual material to good effect: a picture of a Venetian courtesan shows her skirt cut away to reveal her underpants and platform shoes. A character in one pornographic story describes looking at obscene pictures (on the walls of a monastery), and her friend asks (p. 74): "How could you control your desire for a man after seeing all that fucking?"

The essays in this book were originally presented at a conference held at the University of Pennsylvania and, like most conference papers, they vary in quality. At times it seems that the authors are unaware of one another's findings. Robert Trumbach obviously knows a great deal about sex in eighteenth-century England, for example, but some of his arguments

might have been modified if he had thought about Findlen's information on Renaissance Italy, where both sodomites and prostitutes were considered a "third sex" (p. 94).

The book's primary emphasis is on pornography and politics (in Restoration England, during the French Revolution, and so forth), but a number of the authors are also concerned with philosophical and formal questions. The literary aspects of pornography, especially its relationship to the novel, are explored in essays such as Kathryn Norberg's "The Libertine Whore: Prostitution in French Pornography from Margot to Juliette." Hunt's essay on "Pornography and the French Revolution" is especially valuable for its insights into the changing image of women in an age of bourgeois and democratic politics.

It is unfortunate that no art historians collaborated on this book. Although there are numerous black-and-white illustrations, they are not subjected to any consistent visual analysis. For academics, the word may be flesh, but most people tend to respond more immediately to visual stimuli. We are also left in some uncertainty as to why pornography apparently changed in character after about 1800. Hunt says only that "After the end of the Terror, French pornographers' attention shifted almost exclusively to the depiction of sexual pleasure as an end in itself. This shift marked the beginning of truly modern pornography, with its mass-produced text or images . . . Paradoxically, once political pornography had become democratized, it ceased being political" (p. 305).

For all their emphasis on the "truth-telling trope of pornography" (p. 37), many of the essays are characterized by a certain naiveté about sex. It is doubtful whether many people have been attracted to pornography because it functions as political and social commentary, as literature and philosophy. Even prior to 1800, pornography also functioned as commercially produced sexual fantasy. As Angela Carter wrote in *The Sadeian Woman* (1978), pornography is "propaganda for fucking."

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MARIO BIAGIOLI. *Galileo, Courtier: The Practice of Science in the Culture of Absolutism*. (Science and Its Conceptual Foundations.) Chicago: University of Chicago Press. 1993. Pp. xii, 402.

In 1610 Galileo received his appointment as "Primary Philosopher and Mathematician to His Most Serene Highness," Cosimo II, Grand Duke of Tuscany. He was forty-six years old. His anti-Aristotelian Latin research on mechanics was then behind him, while his

writings on sun spots, the moon, and the moons of Jupiter were soon to come, and the great *Dialogue Concerning the Two Chief World Systems* and the subsequent trial that brought him down were over twenty years in the future. Yet the same professional ambi-

tion that drew him from republican Venice to the absolutist Florentine court tempted him ultimately to Rome, where Icarus's wings melted in the glare of pontifical displeasure.

What Galileo wanted from Cosimo is what only a great princely court could give him. His ambition is contained within the terms of the Tuscan court title: he was now to be not just a mathematician but also a philosopher. The distinction was fundamental and consequential. The medieval and early modern *scala artium* ascended from *pi* to *theta*, from practice to theory: mathematics counted in the main as practice, natural philosophy as theory. The discipline that confined itself to charting the regularities of terrestrial and celestial motions was less dignified than that which discoursed of their physical causes. Medieval and early modern universities institutionalized that hierarchy of value and rendered it rigid. Mathematicians counted for less than philosophers, and their remuneration and social standing reflected that evaluation. When Galileo became the Medici's court mathematician *and* philosopher, he shrugged off the shackles of university constraints and made himself into someone who could legitimately speak in mathematical language of the real causal structure of the physical world. By the 1580s, the son of a cloth merchant and professional musician had already ascended to the quasi-bourgeois title of *molto magnifico*, and by the 1600s to the gentle designation of *molto illustre*. As Medici court mathematician and philosopher, Galileo now became *eccellentissimo*, and his stipend of 1,000 *scudi* per annum was equal to that of the highest court official.

The Medici gave Galileo *scudi* and Galileo gave the Medici the stars. Directing the newly invented telescope to the heavens, Galileo announced the discovery of four "stars" orbiting round the planet Jupiter. What else to call them but "the Medicean Stars, in the hope that this name will bring as much honor to them as the names of other heroes have bestowed on other stars"? The *Sidereus nuncius* of 1610 was a well-judged and a well-timed gift, for Medici emblematics made much play with Jovian themes, and the still-new Medici dynasty was aggressively in the market for cosmological state myths to consolidate its power. Galileo's gift gave much pleasure. And once settled as a courtier, Galileo could use Medici authority to establish and secure what Biagioli calls—evidently to avoid the barbarous jargon of the sociologist's "role"—his "socioprofessional identity."

Accepting the Medici *scudi* entailed accepting court conditions for natural philosophy. On the face of things, those conditions were not onerous: Galileo's concurrent appointment at the Tuscan university at Pisa carried no teaching obligations, nor was it demanded that he actually attend the Florentine court very often or very long. Galileo spent most of his time "at court" living as a patrician in a suburban villa, tending his much-loved (and aptly named) *sangiovese* vines. Yet the court that permitted Galileo plausibly

to function as a natural philosopher did not share the professional's conception of what natural philosophy was for.

Although courtiers had use of scholars' knowledge, they were not well disposed to what was taken as scholarly pedantry. The Medici enjoyed philosophical disputation on the condition that it amused and philosophical discoveries on the condition that they amazed. Natural philosophy was, for them, a largely "performative" art: the dispute of 1611 over buoyancy between Galileo and the Aristotelian Flaminio Papazzoni was conducted over lunch at the Grand Duke's table. Princely preferences for pudding-philosophy obliged practitioners to dispute quickly, concisely, and wittily. And, although Galileo and his philosophical colleagues might care deeply about the physical truth of the Copernican system, it is not clear that great courtiers shared such concerns. Patrons encouraged philosophical duels, but felt free to put an end to them when they became awkward or tedious. If philosophical *sprezzatura* was not all, it was, from the Medici point of view, very much. In the court, conversation counted for more than certainty.

Cosimo II was a great prince and a great patron, but the pope was greater still. Two years after Cosimo died in 1621, Galileo's friend Maffeo Barberini was raised to the pontificate as Urban VIII. This was the "miraculous conjuncture" for which Galileo had waited. Copernicus's *De revolutionibus* had been put on the Index in 1616, and Galileo was forbidden to advocate the physical truth of the heliocentric system. Yet the new pope was known to be so taken with Galileo's *Assayer* (1623) that he had it read to him at mealtimes, and Galileo was given six papal audiences in the six weeks after its publication. If Galileo was going to be accounted a truly great natural philosopher, he had to have a system, and it was only the pope who could guarantee the success of the Copernican system whose credibility Galileo now sought to secure. Galileo was now an old man, and this was his last, best chance.

But Rome was a more volatile court than Florence—the Fall of Favorites was a Roman ritual of power—and those who sought papal patronage lived dangerously. Urban was distracted by Spanish intrigues in which Galileo's friend Giovanni Ciampoli was implicated, and, as Mario Biagioli puts it, was going through "a psychologically difficult period" (p. 336), seeing enemies everywhere. The pope needed to take a strong public stand against heretics, and so, a friend of Galileo but more a friend of Catholicism, Urban presided over the fall of his "intellectual favorite."

Galileo's career, Biagioli writes, "was structured from beginning to end by the patronage and culture of a baroque court" (p. 349). So, to the traditional Galileo-as-disinterested-martyr-of-science, and Pietro Redondi's recent Galileo-as-heretic, we now have Galileo-as-scheming-baroque-courtier. It is an entirely plausible portrait, argued with vigor, swagger, and

immense knowledge of Italian court culture. This fashionably "self-fashioning" Galileo suits our academic times. The scientist is here displayed as a political animal in a court context where to be a political animal is to belong to, and to manipulate, networks of patronage. Patronage is treated not just as an appanage of philosophical life but as its lifeblood. Biagioli invites us to see the early modern scientist in just the same way that social historians of art and literature (for example, Michael Baxandall, Francis Haskell, and Stephen Greenblatt) have taught us to see their subjects. Other historians of early modern science have drawn attention to the place of patronage—including William Eamon, Paula Findlen, Bruce Moran, Richard Westfall, and Robert Westman—but none has argued its significance for the development of scientific knowledge with such single-mindedness. That unrelenting focus on patronage as the shaper of scientific careers and scientific knowledge is the provocative strength of Biagioli's study.

Biagioli's central claim is that the Medici gave Galileo not only *scudi* and a "socioprofessional identity" but also scientific credibility. Absolutist patronage, according to Biagioli, enabled Galileo to link the credibility of his astronomical and philosophical claims to the honor of his prince, thus molding truth effectively to the contours of power. Absolutist power could enhance the credibility of observational claims in a way that expertise alone could not. Yet Biagioli's apparent move from intention to effect is only allusively argued. Nor, if more strongly argued, would it be plausible without important qualification. Galileo's cosmological flattery would have done the Medici little good if the Medici were not satisfied that their

court philosopher was a producer of reliable knowledge. And the Medici, to so assure themselves on this point, intermittently sought the opinions of other court and academic philosophers.

Indeed, even though the conclusion is not drawn, this book contains much evidence that the contentment of such other practitioners as Johannes Kepler (at the Habsburg court in Prague) and Christopher Clavius (at the Collegio Romano) was an important condition for the contentment of Cosimo with Galileo. If expert astronomers could not make credible natural knowledge by themselves, neither could courtiers. The legitimacy and interest of a sociological history of science is not at issue here. Reference to professional expertise in making credible knowledge, no less than reference to princely power, is a social phenomenon.

Nor are 1,000 *scudi*, at any exchange rate, equal to thirty pieces of silver. When Galileo became a courtier, he already had in hand a well-developed set of mathematical and philosophical tools, and he had too much invested in them merely to dance to Medici tunes. He tried to use the Medici as much as the Medici tried to use him. It is, after all, in the nature of a "practice" or a "role" not to respond to external pressures in a simple and direct manner. Nevertheless, one achievement of this important book is that historians will no longer be able to sustain the traditional view of "science speaking truth to power," while a mark of Biagioli's occasionally glib argumentative style is the impression that science is solely about "seeking power."

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JAMES MILLER. *The Passion of Michel Foucault*. New York: Simon and Schuster. 1993. Pp. 491.

DAVID MACEY. *The Lives of Michel Foucault: A Biography*. New York: Pantheon. 1993. Pp. xxiii, 599.

It seems as if historians, and especially intellectual historians, have been struggling to negotiate the "linguistic turn" for quite some time now. Twenty years ago, Hayden White offered us "Foucault Decoded" (*History and Theory*, 12 [1973]); we were on our way. And an exciting journey it has been. But where have we traveled, or should I say where has the journey taken us, because along the way, somehow, human agency, authorial intention, even the self, have gone missing—and with them, we have been told, objective truth and meaning. A few years ago in the pages of this journal and as part of an ongoing "Forum" discussion featuring historical meaning, David Harlan (citing Michel Foucault's authority!) could glibly recapitulate what had become by the late

1980s a fairly standard assessment of how intellectual history was being transformed into a history of discourse and how from this new perspective "individuals . . . simply disappear. The history of discourses disperses the historical agent, 'the knowing subject' . . . indeed, this is why individuals have been virtually obliterated from his [Foucault's] histories" ("Intellectual History and the Return of Literature," *AHR*, 94 [June 1989], 590–91).

What a pleasure, then, and not a little ironic, to read these two fine and different biographies whose very individual subject is Michel Foucault. James Miller's book is not merely insightful biography; it is intellectual history of the finest order. It is biography that suggests the autobiographical, a history that

indicates the simultaneously objective and relative character of all meaning and the interpretive and interpreted quality of every life/experience. We read of Martin Heidegger's influence, of Jean-Paul Sartre's enthusiasm for Heidegger, and of Heidegger's scorn for Sartre's metaphysics and humanism, for instance, as contexts for Foucault's life, and not just for his ideas and his intellectual development. Miller refuses to separate ideas and experience. Indeed, he emphasizes throughout that ideas are experiential and struggles with Foucault to actualize this realization. Foucault asserted in 1983 that "at every moment, step by step, one must confront what one is thinking and saying with what one is doing, [with] what one is." And Miller recognizes that "this requires examining the fusion, or perhaps confusion of concept and existence, of dream and reality" (p. 9). In both biography and life, the struggle, at this level, requires moving beyond dualism and dialectics.

Miller uses and criticizes "Foucauldian" critiques. He neither treats Foucault's texts as reflective of or representing contexts (autobiographical, social, sexual, and so on), nor does he deny contextual objectivity and meaning and treat the texts in isolation. Rather, he collapses them, suggesting a meaningful life derived from recursive practices and reflexive self-monitoring. The self (Foucault's) remains objective, yet always under and requiring interpretation, an agent and an intentional phenomenon defining, and further defined by, unintended, outcomes. Miller moves freely from texts to contexts, contexts to texts, elaborating neither's primacy and finally denying their historical difference. The texts mediate Foucault's self-reflexive project. They are where he appeared to and disappeared from others as he created the self he discovered and discovered the self he created. Thus, Miller presents the life and work, the life-work, as one. This proves not a synthetic result of dialectics but a wholistic and practical result of interpretation.

Up until the last years of his life, Foucault struggled to obfuscate how autobiographical his writings were. As Foucault came to admit, each work was part of a search for the self he simultaneously was trying to create and hide. By shaping this enterprise in anti-humanist, anti-essentialist "discourse," he succeeded best at accomplishing the latter aim. What Miller's book suggests is the history of how this hidden self (escaped self?) has been translated by a generation of users of Foucault into no self: for Foucault, or for any of us. Indeed, by interpreting self out of existence, and then by failing any more to search for it, contemporary critics granted Foucault the space and time he needed to find and create who he was. And in the meantime we have reconceptualized, in Foucault's name (among others), self, authorial intention, and history.

It seems that what Foucault intended was not to escape from his self, but rather to avoid others' gazes, and then to appear to disappear, to suggest as pow-

erfully as he could that he was not "there" to be seen so that others' interpretations would not define him. It is in this context that Foucault's continual aversion to Sartre's humanistic existentialism can be read, as both Miller and David Macey imply.

Foucault intended to appear unintentional and thus to disappear. Yet Miller misses the point, I believe, when he argues that "Foucault's experiment in anonymity was, if not a hypocritical farce, then at least a comic failure" (p. 162), because it was not anonymity he sought, but rather control over the interpretive process that makes anything, including one's self, meaningful. And so there also was a success, as Miller recognizes, because the self-effacement left only the name Foucault for people to conjure with. Foucault protected his "real" self by presenting only a "nominal" one. And to his credit, Miller avoids the temptation to follow the nominalistic, role-playing Foucault and presents in his turn a realistic Michel Foucault.

Miller notes that Foucault spoke most tellingly about self and about his self when discussing homosexuality. He "simultaneously affirms the possibility and denies the likelihood, of an identifiable 'self' emerging as a sort of existential 'work'" (p. 258). Yet one might argue that Foucault denied neither self nor identity. Rather he struggled to resist what he understood to be the sheer objectivity of the self's significant subjectivity, to deny the self-sign finally to signify and thus to define himself. Foucault remained caught in the very dualistic world view he consciously tried to avoid and, as with most contemporary theorists associated with postmodernism and poststructuralism, he unreflectively assumed a dyadic semiotic that reduced all meaning to some understanding of the relationship between sign and signified. So he worked to control how he would be interpreted. Without a dualistic ontology, Foucault might have had confidence in what he experienced, that the self was objective and relative at once, that all objects required interpreters, and that all interpreters were always under interpretation. His need to control others' gazes, to serve as both the sign and the interpretant of his own objective being for fear that any other interpretation could be definitive, would have been unnecessary and his reflexive project of self-creation might have proceeded less anxiously, perhaps less dangerously.

According to Miller, Foucault organized *Madness and Civilization* "around the nagging question posed by his singular *daimon*: 'How did I become what I am and why do I suffer from being what I am?' Searching for the roots of his own mad fascination with death, he was creating a 'work' of madness, as well as *about* madness" (p. 109). Elaborating this interpretation, Miller organizes his book around the question of Foucault's death and the ethical character of his life-work, around the problematic nature of ethics in a world stripped of "foundations," a world, that if without individuals, is seemingly without individual

responsibility. All of Foucault's works examine the classical question of the ethical, the good life, Miller maintains, and yet Foucault only understood what he discovered and created as he wrote his last books and lived his last years and his death.

Foucault's last books, written in the last year of his life, differed from his earlier ones not because they were autobiographical—all his work was, he finally noted—but because in them he reinserted what had formerly been occluded: the subject, the self, the self searching for the truth about itself.

In these books, writing became an ordeal as Foucault would have it, a source of self-valuation, an interior gaze that shames one into acknowledging himself and creating himself anew, even as it attempts to emancipate the self from an other-imposed gaze. But could one only write for and to oneself? Miller glosses that last work as private confessions. But as Foucault wrote in the first volume of the *History of Sexuality*, power and control reside with the one who hears the confession, not the one who speaks it. Was Foucault finally writing to himself? The wonderful irony in this is that all who comment on the last books mention how wonderfully lucid for the first time Foucault's prose is.

Unfortunately, trying always to essentialize the existential, to universalize the contingent, to authenticate his self, Foucault was/became not like a comic, postmodern character, but like the clearest tragic characters of the modern theater, inscribed when the modern project was raw and most problematic: Cordelia and Coriolanus. From 1953, when he called suicide "a moment of potential 'authenticity,'" to 1982, when he identified suicide as the conduct most worthy of "careful thought"—"one should work on one's suicide throughout one's life"—Foucault struggled in his works and life to create and cancel himself, simultaneously, freely. As his long-time friend and lover Daniel Defert poignantly stated, the philosopher "made an ascetic work of himself, and it was within this work that his death was inscribed" (p. 351).

One must be careful not to romanticize the life. There was and remains far too much romanticism and idealism hidden in the postmodern message. The overwhelming feeling I took away from Miller's wonderful book was fear. It frightened me, haunted me. Why are we so afraid to be responsible selves when we so desperately need, today, to be so? Perhaps therein is the explanation. If I may carry on the interpretive process, Foucault, it seems to me, spent his life afraid of the freedom he so passionately sought. All he finally allowed was the contingent freedom to play the game of life. As Miller paraphrased Foucault, power puts in play "a dynamic of constant struggle. There is no escaping it" (p. 352). Everything is dangerous: truth, crossing the street, doctors, AIDS. Why fear any of these, Foucault maintained. But instead, he feared the burden of exercising freedom as if it were not contingent.

David Macey's study is also a good book. Macey

chronicles Foucault's adult life, accounting for and insightfully discussing every major and many lesser works, tracing his professional career, and examining his political activities. He leads us step by chronological step through Foucault's public life and works, keeping them separate, even thematically so. He never attempts to construct a Foucault, or his "life-work," as Miller does, and he never attends to Foucault's understanding that life and *oeuvre* are one; that there is no life without an *oeuvre*. Or if he does so attempt, he seems to suggest that this is best displayed positivistically: lay the cards on the table and you have the deck. Much of the time this makes for informative, useful biography, and I do not want to demean Macey's work by suggesting that it be read only as a complement to Miller's, but I found it most beneficial in that service. Indeed, Macey's scrupulous research and detailed textual exegeses provide the type of documentary support that is often missing in Miller's book.

Yet too often as I read about another essay or what Foucault was beginning to work on in a certain May in a particular year, I felt that Macey's "objective" approach had rendered Foucault lifeless—a nominal catalogue of barely related ideas and experiences—indeed a life without a subject. Macey's title is telling, I believe. He offers up the lives, not the person. It is as if he rearticulates a nominalistic Foucault, an umbrella name covering a variety of distinct and perhaps discontinuous roles. He attends as carefully to Foucault's political experiences, as he does to his writings, but dichotomizes them so clearly that he provides no sense of the Foucault he quotes as inseparably linking life and work(s). And by so doing, Macey almost totally avoids interfering in Foucault's private life: his sexual activities, his fascination with pain, torture, and death, and his longstanding interest in suicide. This is a very polite biography.

Most disappointing is that Macey's claim that "Foucault's life was also the intellectual life of France . . . His biography is also of necessity an intellectual history of his times" (p. xi) remains an empty gesture. One does learn about the life of an intellectual in post-World War II France, the schooling, the tests, and the intellectual rivalries, but the concerted effort to keep texts and contexts separate and to problematize neither insures that, at best, Macey has written the biography of an intellectual.

Macey's work elaborates in detail an authoritative and intentional Foucault, corroborating Miller's analysis. Foucault plotted over and with publishers, used his power to pressure reviews and censor articles, deleted early works from his resume, thus reinventing his authoritative self. He left specific instructions with Defert so that there would be no posthumous publications—he thought Max Brod a cad. Indeed, he even wrote, under a pseudonym, the essay on "Foucault" for the "Dictionary of Philosophers" in France.

More openly than does Miller, Macey recognizes Foucault's ongoing struggle against Sartre's "gaze," against any other's interpretive or evaluative power.

For the reedition of *Naissance de la clinique* in 1972, Foucault rewrote an informative sentence altering "a structural study" and featuring "discourse." Macey states what Miller repeatedly suggested, noting "Foucault's marked tendency to obliterate elements of his past in order to redefine himself in terms of his concerns of the moment, and thereby to frustrate attempts to situate him in absolute terms through the exploitation of an interplay between identity and non-identity" (p. 133).

What these biographies, together, make clear is that Foucault was not afraid to change his mind about issues intellectual, political, or personal, and that he often did. To render his thoughts or theories from any particular text or two, even from his very theoretical texts, or to use his ideas rigidly to establish a methodological approach to history or any other subject, is to give the lie to both his thoughts and his

life; indeed, to his life-work. This does not make his life either one of continual change or a series of discontinuities. It neither suggests that we cannot find a Foucault to know nor that Foucault, finally, is but a name given to a number of discrete roles. It supports rather what Foucault recognized and struggled to control—that all meaning, even meaning about self, is simultaneously objective and perspectival. While a self may be and may appear to be a series of roles, the self still is—even if it is only what it appears to be.

It seems safe to say that historians are confidently finding their way again toward objective meaning and truth. With these books in hand, many historians may find that the Michel Foucault invoked for so long to decry such presumptuous practices might now serve as a useful guide for their complex endeavors.

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DAVID LEVERING LEWIS. *W. E. B. Du Bois: Biography of a Race, 1868–1919*. (A John Macrae Book.) New York: Henry Holt. 1993. Pp. xiv, 735. \$35.00.

Reading David Levering Lewis's biography of W. E. B. Du Bois reminded me of a conference I attended some years ago. One of the panels, dedicated to the works of Du Bois, attracted a large audience. Presenting papers were a number of young and promising African-American historians, many just out of graduate school, and all in one way or another aware of the debt they owed Du Bois for his pathbreaking studies of the black experience. The commentator was the radical, C. L. R. James, historian of the Haitian Revolution and Du Bois's co-worker in the post-World War II Pan-African movement. While doffing their collective intellectual caps to Du Bois, all the presenters attempted a critical and dispassionate reevaluation of his work. As I recall, some came close to dismissing him as a petit bourgeois intellectual. That did not sit well with James. When his turn came to comment, he sat silently for a moment, apparently gathering his thoughts. But some of us knew what was coming. Taking his watch off and placing it on the table, James turned to the panel, and in that way of his which brooked no uncertainty, offered a peremptory evaluation of the papers: "You know nothing about Du Bois. I will tell you all there is to know about Du Bois in thirteen minutes." James was known to keep a promise. Lewis can be forgiven for taking a little longer to tell us all there is to know about the first half of Du Bois's life and, like James, for telling it with a commanding conviction. This, the first in a projected two volumes, covers the years 1868–1919; the second will trace the second half of Du Bois's long life to his death in exile in Ghana in 1963.

This is no conventional biography; it is as it is most aptly described in the title, a "Biography of a Race."

Nothing and no one escapes Lewis's scrutiny. Like his subject, Lewis leaves nothing to chance. Even on the rare occasions when the records are silent, Lewis has a knack for employing reasonable speculation to round out his portrait or analysis. It is without doubt the tidiest biography I have read. Lewis puts to rest the old adage that good biographers are like bad mechanics; when they are finished there are lots of pieces lying around that they ought to have used but have no idea where they fit. Lewis makes things fit and never once overburdens the reader or his subject with detail. Although he is careful to keep Du Bois center stage, there exists a perfect and constant tension between all the characters and events in this high drama. The vignettes are gems of precision. All the characters are rounded and fully developed; there simply are no rough edges. There is, for example, Du Bois's major protagonist, Booker T. Washington, who gets fair and balanced treatment; so does John Hope, Du Bois's friend and president of Morehouse College in Atlanta; and so too does Henry Sylvester Williams, the barrister and organizer of the first Pan-African Conference in London in 1900.

The fabric of this biography is spun from a variety of tightly woven yarns, some intellectual, others organizational, all dipped in the brine of American racism. We see a young intellectual influenced by the heroic traditions of the Scottish Thomas Carlyle and the German Otto von Bismarck, as well as by the home-spun traditions of African-American thinkers like Alexander Crummell and Martin Robison Delany. Crummell, Episcopal priest, Cambridge graduate, Liberian colonizationist, and advocate of African uplift, left a lasting impression on Du Bois.

They first met when Crummell visited Wilberforce University, where Du Bois was teaching. There and in his involvement with the American Negro Academy, inspired by Crummell, Du Bois came to appreciate the significance of Crummell's notion that those with the skills had a divine obligation to work for the improvement of the race. Du Bois would write of Crummell's influence in a moving essay later included in his masterpiece *The Souls of Black Folk* (1903). Crummell, Delany, and other progenitors of twentieth-century black nationalism, Lewis argues, left a lasting impression on Du Bois's thinking. Like them, he believed assimilation was not an option available to African Americans. This, Lewis insists, was the genius of *The Souls of Black Folk*: it conceived of the destiny of the race "leading neither to assimilation nor separatism but to proud, enduring hyphenation" (p. 281).

Lewis is right when he argues that there existed a tension between assimilation and separation throughout the nineteenth century. But although there is a thorough analysis of nationalism (separation), he has little to say about the intellectual antecedents of integration (assimilation). Like many contemporary ethnologists, Du Bois believed that races were endowed with unique characteristics, bestowed by nature, which they contributed to the mix of civilization. These views found their most advanced expression in "The Conservation of Races," which Du Bois first read at a meeting of the American Negro Academy in 1897. At least in this instance, Du Bois broke no new ground. Much of what he had to say about the contributions of races to an evolving civilization are to be found in the works of such antebellum intellectuals and activists as William G. Allen, Professor at McGrawville College, New York; Dr. James McCune Smith, prominent New York physician; and the Rev. Samuel Ringgold Ward, radical abolitionist. For Lewis, the quintessential assimilationist was Frederick Douglass, the counterpoint to separatists like Delany. Lewis seems to have thrown his weight behind the conclusions of a number of recent studies that, in their praise of nationalists like Delany, have been almost dismissive of Douglass the integrationist. This has found its most fervent expression in Vincent Harding's brilliant study, *There Is a River* (1981). To paint a picture, as Lewis does, of Douglass the patriarch of integration sitting "on his Cedar Hill estate above Washington, white second wife by his side, roaring, 'Assimilation not isolation is our true policy and our national destiny,'" is to do Douglass a terrible disservice. The race of his second wife had nothing to do with his conviction that equality would come through integration. He had roared similarly from his much simpler home in Rochester in the 1850s, while married to his black first wife.

But these are relatively minor quibbles with what is a comprehensive and finely written biography. Lewis is always in command of his subject, whether it is discussing the complex nuances of late-nineteenth-century German or American intellectual traditions,

or probing into the clouded background of Du Bois's ancestry. There is possibly no higher praise for any biographer of Du Bois than to say that he has matched his subject, the master of the lyrical emotive prose, with a style that at times is almost rhapsodic. Here is Lewis's description of the body of Du Bois as it lay in its coffin: "Du Bois lay deep in his burnished vessel, bronzed flesh encased in bronzed metal, craved and light-suited, his features even more refined in death, the finely spheroidal cranium and trimmed Wilhelmine mustache and goatee completing the effect of assured apotheosis" (p. 5). It is not always that such penetrating studies as this can be described as an easy read. But Lewis manages to take his readers along an exciting excursion from the riveting introductory chapter, "Postlude to the Future," to Du Bois's return from the second Pan-African Congress in Paris and the "Red Summer" of 1919. The "Postlude," rounding the circle of a great life, uses the historical coincidence of the great March on Washington in 1963 and Du Bois's death to introduce us to the subject as if to confirm the old saying that nature abhors a vacuum: rebirth must fill the gap created by a departed spirit. We come to know Du Bois first in death in exile in Ghana, praised by all the great anticolonial leaders for his uncompromising struggle against oppression, yet reviled by his own government and many in a country he had prodded and cajoled to live up to its highest principles.

We also come to know Du Bois the master historian, sociologist, and polemicist. Lewis introduces us to the conditions (the times), the men, and the intellectual traditions that went into all of Du Bois's work, histories like *The Suppression of the African Slave Trade to the United States of America, 1683-1870* (1896), sociological studies like *The Philadelphia Negro: A Social Study* (1899), and what Lewis rightly calls the epochal study, *The Souls of Black Folk: Essays and Sketches* (1903).

One leaves this volume anticipating the changes that would move Du Bois, the admirer of Carlyle and Bismarck, an elitist, a political conservative, deeply suspicious of the white working class, to adopt and advocate radical solutions to the conditions of African Americans. Although Du Bois abhorred physical violence, he was committed to the notion that the bastions of racism had to be encircled and assaulted until they crumbled. The "violence of ideas, the insurgency of attitudes, the rupture of deference, and the constitutional assault upon oppressive institutions were the essential conditions of black racial liberation" (p. 478). Du Bois would become less metaphysical in his later life, but no less committed to the destruction of oppression. The anticipation of later volumes can be a daunting proposition when one has set such high standards. But if any one can come up to par, Lewis can.

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SERGE GRUZINSKI. *The Conquest of Mexico: The Incorporation of Indian Societies into the Western World, 16th-18th Centuries*. Translated by EILEEN CORRIGAN. Cambridge, Mass.: Polity; distributed by Blackwell. 1993. Pp. vi, 336. Cloth \$49.95, paper \$19.95.

The endless fascination with the conquest of Mexico arises from our lingering, and well-founded, suspicion that we do not comprehend the reaction of the indigenous population to the onslaught of a completely foreign culture. Although we can grasp the military essentials of the invasion without much difficulty, the much more important process of the reorienting of aboriginal cultures by the Indians themselves to fit the perceived needs of the Europeans, and thus to assure the survival of the defeated, is another matter. Our understanding of psychological trauma, of short and long-term reactions, is at best elusive, murky, and, in a number of important areas, seemingly opaque. We comprehend to a reasonable degree only one side of the epic struggle for domination. Contemporary European accounts have left a record of their objectives and methods that make a historical reconstruction relatively simple. The victor, of course, writes history, while the defeated live it. Reality, as reported by Spaniards, was not the reality endured by the various Indian groups whose own *imaginaire* registered their private inner world; one that might appear bizarre to the European had they been able to penetrate the aboriginal mind. Mental confusion, adaption, and transformation, expressed in ways that are themselves difficult to identify, are reactions that must be probed if we are to begin to comprehend the conquest of the Indian. The inner emotional life-and-death struggle played out in each individual mind of the vanquished represents a heroic, even epic, chapter in human endurance.

The sheer magnitude and daunting difficulty of psychological reconstruction has deterred all but the most daring. Serge Gruzinski is one of a small, intrepid group. The less adventuresome settle for charting the transformation of the conquered, including their acceptance of European dress, religion, language, food, alcohol, and new economic activities. Although such elements are important, and certainly offer clues to the inner life, they remain outward manifestations. To go below the surface in pursuit of the mind requires a delicate, sensitive, and imaginative touch. Gruzinski has ably demonstrated such traits. At the same time, he resists the temptation to overreach, often suggesting that the reader exercise prudence in drawing conclusions. The result is an exciting speculative work, but one that is within rational bounds. It is a study best appreciated by readers well grounded in the field and willing to give it a careful reading. Gruzinski's work is an intellectually interactive one, in that the points he seeks to establish stimulate the reader to think of many different variations and other possibilities. Such books

indeed appear to have an intellectual life of their own beyond the original intent and control of their authors.

The overarching question of what culture is, how it is attacked, changed, defended, modified, or reinvents itself, is an absorbing one. Is it a purely psychological construct relatively immune to external factors under normal conditions? What are the consequences of shock and disaster, such as conquest, on a culture? Is it possible to be cut adrift without experiencing a collective and individual madness? It is obvious that culture is more of a laminated affair than a monolithic creation. Class, occupation, and other factors result in internal differentiation resulting in subcultures that function within accepted cultural boundaries. The intersection of subcultures with the ideal generally occurs only at certain political, social, and religious levels deemed crucial. The elites strive to set the parameters, employing their superior knowledge and status to coerce a society's subcultures to acknowledge their limitations and demonstrate adherence to the overall culture. Acceptance of the notion that a subculture functions within a context set by others, even if only dimly and imperfectly perceived, results in a useful vulnerability. Constantly reenforced inferiority holds a society's various elements in place. Priests and nobles hold the keys to spiritual and political authority within a cultural structure.

Scholars generally accept the view that preconquest societies remained stratified even as they became more complex. As time moved toward that fateful moment when the first Europeans landed on the coast, social rigidity still characterized Indian Mexico. Social mobility, although not impossible, required extraordinary circumstances or luck. The acquisition of wealth by those at the lower levels that had dissolved such stratification in Europe could be discerned in Mesoamerica, but it remained at a nascent level. The *Pochteca*, the hereditary merchant class, eventually would have broken through, perhaps with revolutionary consequences for the nature of Indian society and culture.

The essential rigidity of Indian Mexico at the time of the conquest made it possible to decapitate this society and substitute a new elite initially based on force, subsequently replaced by culture. The rapid introduction of European culture provided the means to control and press demands on each individual. The old guardians of culture, discredited by defeat, struggled to find a new niche, eventually becoming themselves part of a Spanish subculture. Indian nobles, offered incorporation into the new society of the conquerors, had the choice of either

rebellion or collaboration and absorption. Those who chose the former chose death; those who accepted life abandoned their people. The Indian lower classes, and their various subcultures, now experienced the full onslaught of European culture. They became in effect cultural refugees, the detached remnants of a suddenly illegitimate structure. Cultural survival and the retrieval of necessary cultural elements abruptly became the task of those on the bottom, but who were now deprived of their elite and the superior knowledge they had employed. Missionary friars proved relentless foes of the old order, insisting on fatal changes, building churches on sacred ground, gathering groups together, and calling into question virtually every mental construct that had constituted reality before the conquest. The least prepared, those at the lower reaches of a stratified society, had to rally themselves, decide what could be accepted, and what must be held on to at all costs. A new reality had to be created that would make Indian survival possible. This is what provides the drama of Gruzinski's work.

The weapons of the new culture could be used against the Europeans, yet at the same time these weapons required acceptance by the Indians, a process that itself was a concession to what Indians sought to reject. Alphabetical writing, a major European cultural instrument, captured and reduced Indian languages to paper. Words changed concepts and narrow definitions. Oral traditions and pictographic representations lost ancient meaning while gaining new elements. Portrayal of space on Indian maps and pictographs changed radically and old symbols disappeared along with their conceptual reality. Nevertheless, writing in the hands of Indians could be used to defend local authority, land, and perhaps even expand such claims. Utility made the Indian avid learners. Alphabetical writing posed no great difficulty for Indian students. A mere decade after the fall of Tenochtitlán in 1521, some 600 young Indians had become proficient in reading and writing. Convent libraries provided ready access to the European imagination both in its written and pictograph form. Armed with elements of European culture, the Indian proceeded to defend the essential elements of their own. Missionary friars, who had earlier believed that their spiritual beliefs would transform and mold the Indian as they desired, suddenly sensed that the tide had turned against them. A sense of betrayal and anger robbed the missionary effort of its vitality. The colonial church, increasingly institutionalized, became an enclave and sanctuary of European culture amid a no longer

pliable mass of Indians who had drawn a line beyond which they would not go.

Reading and writing skills developed by the Indians now became in the eyes of the Spaniards instruments of the devil, and the "truth" drawn by their cultural apprentices approached blasphemy. The "paper that speaks" spoke to a different historical experience, resulting in a mental construct seemingly beyond European comprehension. Latin, rhetoric, philosophy, and history somehow became dangerously different in the hands of the supposedly "defeated." Even worse, Indians managed to "know the origins of our existence," including Spain's own pagan background and the process of conversion in Europe. In 1585, an alarmed archbishop of Mexico pronounced against instructing Indians in such crucial areas of knowledge. The fact is that the Indians had identified the pressure points of European civilization and developed spiritual and intellectual techniques to defeat its extreme demands.

The miraculous apparitions of the Catholic virgin that occurred throughout Indian New Spain represented cultural skirmishes, snuffed out in one region only to ignite elsewhere. Our Lady of Guadalupe in the end became an acceptable truce for both sides. At the level of the saints—*Los Santos*—the compromise favored the Indian. Although many Europeans viewed *Santos* as evidence of thinly disguised idolatry, the saints represented a clear spiritual victory for the aboriginal supernatural world. As Gruzinski notes, *Santos* provided "anchoring points" in a reconstructed Indian reality. Indians drew psychological strength from their own creation. The role of the *Santo* in Indian communities set limits on the authority of the Catholic priest. Priests wisely accepted such limits or faced united opposition, even violence. By creating their own internal spiritual structure, within the overarching one introduced by Spain, Indians returned a acceptable measure of legitimacy to themselves.

The long struggle, under cover of a degree of acceptance of the imposed culture, has yet to be recorded, let alone understood. What is clear from Gruzinski's work is that new and innovative perceptions in the hands of a scholar can begin to uncover a fascinating reality, the existence of which has been long acknowledged but never deeply understood. It is also obvious that the conquest of Mexico will continue to engage our interest, perhaps to be joined with an Indian *reconquista*.

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Reviews of Books

GENERAL

DAVID GROSS. *The Past in Ruins: Tradition and the Critique of Modernity*. (Critical Perspectives on Modern Culture.) Amherst: University of Massachusetts Press. 1992. Pp. xi, 175. \$25.00.

This book is a gem. Its subject is the relationship between tradition and modernity; more specifically, how feasible it might be to mobilize certain aspects of tradition for the purpose of rethinking the problems of modernity. For it would be difficult to deny that today scholars have increasingly come to view modernity as problematic, even deeply so. What a distance we have traveled since the social-scientific vogue of "modernization theory" in the 1950s.

David Gross tries to eke out a middle course between two unreflective extremes. One extreme contends that traditions are restrictive and repressive, and that the further we move away from them in the direction of modernity the better off we will be. The other claims that modernity leads solely to fragmentation, devastation, and anomie, and that our best course would be to return to a traditional mode of life with all speed.

Gross's position is that, by recapturing selected aspects of tradition as they live on—however precariously—in the historical present, we are able to gain much-needed perspective on the predicament of modernity. His book is an exercise in redemptive criticism. He recognizes that today the semantic potentials of tradition often survive only in fragmentary form. Consequently, they need to be patiently resuscitated by historians and critics.

To be sure, traditions can be stultifying and dogmatic. But they have also provided valuable resources of social solidarity and meaning, a sense of belonging and place. They are the repositories of values that have too often been given short shrift by modernity's apparent orientation toward progress at all costs. It is the exquisite balance Gross strikes between the values of tradition and modernity that makes his book such a salutary intervention in the recent, contentious debates concerning these themes.

According to Gross, the forces of market, state, and "culture industry" have combined to render tradition fragile under modern conditions of life. One of the

dangers of these media is that they attempt to restore tradition artificially. They seek to provide an ersatz for lost traditions, but solely on their own terms.

Gross is under few illusions about the viability (let alone the desirability) of a return to tradition. As he observes, "One of the consequences of modernity is that the connection between the need to feel anchored or 'at home' and the availability of tradition to satisfy this need, has been broken" (p. 90). Thus, he fully accepts the fact that, for better or for worse, contemporary social criticism must make its stand from within the modern world. While valuing the capacities of tradition to illuminate the failings of the historical present, his book is free of all nostalgia.

Gross's undertaking is modest in scope. His book is written after the manner of the French *essai*. Nevertheless, he has provided us with an excellent book: a highly stimulating exploration in intellectual history and social analysis.

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WILLIAM A. GREEN. *History, Historians, and the Dynamics of Change*. Westport, Conn.: Praeger. 1993. Pp. x, 260. Cloth \$49.95, paper \$18.95.

At a time when it is fashionable to speak of the end of "grand narratives" in history, this book by William A. Green is a refreshing corrective to facile postmodernist invocations of the "linguistic turn." Certainly there is justification in current criticisms of "scientific history" (Lawrence Stone) and in the turn to microhistory with its concern with life experiences instead of structures and processes. Yet we cannot overlook the fact that we live in a world undergoing fundamental structural changes. Green proceeds from the assumption that "the facts of history have significance only when they are integrated into an organic concept of change," and he is concerned with the study of the "great theoreticians" who "have attempted to understand the past by discovering the process through which meaningful change occurs" (p. 204).

The theorists Green discusses have all sought explanations for the emergence of the modern world. Green examines six types of explanations: the "com-

mercial model" (Adam Smith); the "demographic model" (Thomas Malthus); the "Marxist model"; the critique of the materialist assumptions of the previous three models (Werner Sombart and Max Weber); conceptions of a world-system (André Gunder Frank and Immanuel Wallerstein); and, finally, recent environmental theories. Despite basic differences in their explanations of the "dynamics of change," all the theorists here "judge modernity to be synonymous with capitalism" (p. 195). Whereas Marxists explain the rise of capitalism in terms of developments in a European core, Frank and Wallerstein argue that capitalism was only made possible by the exploitation of a periphery outside the European metropolis.

While formulating a theory of modernization, Green is careful to avoid evolutionary conceptions that still characterized modernization theories of latter-day apostles of "industrial takeoff" such as Walt Rostow. At the same time, Green is very much aware of the limits to growth and the inequalities of capitalist development. A particularly interesting section of the book concerns Latin American *dependencia* theories and their critical implications for modernization theories. For the accumulation of wealth in the metropolis has been accompanied by the immiseration of the periphery, a process that he believes, as do Frank and Wallerstein, is inherent in the growth of capitalism. "Evolutionary theory," citing Frank, "was a fiction" (p. 137). The center-periphery conception presumes a world-scale social structure that is predicated on the permanent disequilibrium between the developed core and the underdeveloped periphery.

Within barely two hundred pages of text, Green has presented a readable and at many places critical analysis of historians and economists who have analyzed the problems of emergent capitalism in order to arrive at a "well formulated understanding how change occurred at the macrohistorical level" (p. ix). This is a broad analytical overview of theories of change from the eighteenth to the late twentieth century. Yet in its almost exclusive focus on economic history, the book also suffers from a certain narrowness. Green may be right that "history as a study of society cannot proceed without theory" (p. 7), but he has perhaps too closely identified theory with grand theory. Although I agree with Green about the need to analyze the world in which we live with the tools of social-science theories, I believe that he should have given more space to the critique that the theses, theories, and approaches he presents have undergone in recent historical thought.

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PAUL COSTELLO. *World Historians and Their Goals: Twentieth-Century Answers to Modernism*. DeKalb: Northern Illinois University Press. 1993. Pp. x, 315. \$30.00.

"To confront world history is to confront the ultimate questions of human destiny" (p. 213); so argues Paul Costello in his systematic analysis of seven twentieth-century world historians, all of whom satisfy two criteria. Each has sought to encompass the whole of human experience in a grand theoretical construct, and each has undertaken a concrete study of the history of the world from the beginning of historical time. Costello's subjects are H. G. Wells, Oswald Spengler, Arnold Toynbee, Pitirim Sorokin, Christopher Dawson, Lewis Mumford, and William H. McNeill. At no time does he evaluate, individually or comparatively, either the extent or the reliability of the factual evidence employed by these world historians. He is concerned with their motivations, visions, philosophical orientations, intuitions, and schematics.

His subjects have much in common. All were or are citizens of the West, for, as Costello reminds us, no world history has been written from a Chinese, African, or Indian perspective. All considered Western civilization to be in crisis, uprooted from its ancient moral and religious moorings while it greedily and soullessly lurched toward a dreadful apocalypse, one that could manifest itself suddenly in nuclear war or gradually through world-wide ecological disaster. Most of Costello's historians passionately lament the rapid growth of materialism and mechanization. All fear the influence of technology on human liberty and on social values. All decry the rise of rampant individualism, and all conceive that humankind's expanding material power has not been paralleled by an expansion of wisdom or tolerance. Acknowledging that Westerners have lost their sublime faith in progress—to no small extent, an effect of World War I—these historians have sought new meaning for civilization. All have concluded that only through the creation of new mentalities and social organizations can our world escape the abyss.

Costello introduces his cast of historians with a brief but highly useful overview of Western metahistorical theorizing from the Bible through Friedrich Nietzsche. Nietzsche provided the seedbed for twentieth-century metahistorians, and his intellectual presence is felt throughout this book. It is he who pronounced that God was dead; that Western culture, having been shorn of its myths, was adrift; and that civilization was spiritually famished and frantically fumbling through other cultures in search of a deeper knowledge with which to create a new mythic home. Twentieth-century metahistorians have been driven by the need to create meaningful historical myths, to reconstruct a purposeful historical foundation for modern men and women, and to seek ways to rekindle our shattered faith in progress. Their enterprise is philosophical, even spiritual. Employing historical evidence—sometimes without discrimination, consistency, or order—they have assumed the mantle of prophets, chastising society for its manifold corruptions while groping for the means of salvation.

Of the historians studied, only McNeill, indisput-

ably the most professionally rigorous of the group, pursues a strictly linear and progressive view of history. Even McNeill's optimism is hemmed in and hedged about with historical insights that might cause a less irrepressible optimist to posit a more pessimistic view of the future. Spengler, in contrast, rejected progress and adopted a concept of closed cyclical patterns in history. His masterwork, profoundly influenced by Nietzsche, was a despairing attack on the civilization of the West at what he perceived to be its ebb tide. From the 1920s, most metahistorical writing has involved a response to Spengler. With differing assumptions and emphases Toynbee, Sorokin, Dawson, and Mumford have also advocated cyclical metahistorical patterns.

Costello introduces each metahistorian with a general statement defining his philosophical orientation and providing a brief biographical summary. This is followed by a more thorough analysis of each historian's philosophy of history as well as his interaction with the intellectual forces of his time. For each subject there is a brief concluding section that treats the most important criticism that these authors received from contemporaries as well as critical reflections by Costello on their work. The format works well. The reader is repeatedly impressed by linkages between the life experiences of these historians and their theoretical orientations to history, and Costello conscientiously points out pertinent relationships between the views of his seven subjects.

The concept and the plan of this book are worthy. Commencing with Nietzsche's assertion that Western peoples have been stripped of their myths, it concludes by exploring McNeill's brilliant and candid commentary on mythistory. Costello's bibliography is extensive; he exploits it creatively. One would be hard pressed to challenge either his diligence or his scholarship.

Nevertheless the book cannot be recommended to undergraduate readers or to any but the most indefatigable graduate students and professional historians. It is presented in an obfuscating prose that requires perpetual rereading to fathom Costello's meaning. Endless examples might be cited. One will do. Referring to Toynbee, Costello writes: "Challenges must be perpetual and pulsate recurrently within the spiritual lifeblood of the civilizational cliff climber to prevent the arrestation that follows a tour de force response that leaves no challenge unanswered" (pp. 80–81). Although an author must assume ultimate responsibility for the aesthetic quality and basic clarity of his or her prose, a press should exercise sufficient initiative to demand clarity and readability, if not elegance, from its authors. This has not occurred in the case of this book with the result that a worthwhile intellectual enterprise will receive—and should receive—less attention than the topic and Costello's insights deserve.

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MATTHEW H. NITECKI and DORIS V. NITECKI, editors. *History and Evolution*. (SUNY Series in Philosophy and Biology.) Albany: State University of New York Press. 1992. Pp. viii, 269.

Assuming that studies of evolutionary biology and human history deal with similar problems and employ parallel processes in their resolutions, does it follow that the methods used by biologists and historians bring to light comparable knowledge? What are the real differences and similarities between their disciplines? These are the questions discussed in the ten essays from the proceedings of the Spring Systematics Symposium held in 1989 at the Field Museum of Natural History, Chicago, where Matthew H. Nitecki and Doris V. Nitecki are curators in the department of geology. They have organized the papers in this volume under three headings: "Methodologies of Historical Explanations," "Historical Explanations and Evolutionary Biology," and "Historical Science and Philosophy of History."

Matthew Nitecki's introduction, "History: La Grand Illusion," sets the tone for the papers that follow with his observation that, since history is a human invention, there was no history before us, but geology and paleontology testify to changes over time of the earth and the evolution of living organisms. Interpretations of evolutionary history necessarily correspond to the actual events occurring in human activity or in nature. Models about evolution reflect ever-changing concepts in the political-cultural milieu, and the natural sciences are no better prepared to escape cultural biases than are the social sciences, including history, particularly when it is recognized that the ideas of a few selected individuals stand for the state of the art and status quo of their disciplines when defining their models of history and evolution.

The authors of the essays have eminent qualifications in the history of biology (Garland E. Allen and Robert J. Richards), philosophy of science (Marc Ereshefsky, David L. Hull, Rachel Laudan, and Michael Ruse), ecology and population biology (Robert Boyd, Peter Richerson, Douglas J. Futuma, and Lawrence B. Slobodkin) and vertebrate paleontology (David B. Kitts). Human evolution and biological diversity are discussed in these papers, thus the omission of a contribution by a paleoanthropologist or biological anthropologist is curious. Some historians may react in the same way to a lack of representation of their discipline, as these omissions serve to perpetuate the conceit that practitioners of other fields of study are best able to penetrate the turgid problems of human evolution and history.

The essays do not advance a single point of view or narrow consensus of methodological approaches. Laudan concludes in her paper "What's So Special about the Past?" that "The investigation of the past does not have a set of common features that sets it off from other forms of inquiry and, hence, there is no reason to think that biologists can learn more from

the philosophy of history than they can from the philosophies of other sciences with cognate epistemic problems, be they sciences historical or non-historical" (p. 65). Ereshefsky, however, seeks to locate the truly distinctive historical nature of evolutionary theory that distinguishes it from the physical sciences because of the ontological nature of biotic taxa: "extensive sequences of objects which can transmit information with great fidelity. There are no comparable sequences of natural objects in the physical sciences. The only sequences which parallel taxa in this way are the social groups studied by historians" (p. 97). Kitts considers differences between historical geology and paleontology in his paper "Nomothetic Paleontology." Ruse traces the vestiges of Louis Agassiz's three-fold parallelism of the order of living things, ontogenetic development of individual organisms, and history of life as seen in the fossil record as these "laws" survive today in evolutionary theory. In the penultimate essay, "History as Science and Science as History," Allen demonstrates the striking similarity of many opinions held by Charles Darwin and Karl Marx with respect to evolutionary progress and how a study of this juxtaposition of biological and political concepts may illuminate modern historical methodology.

The editors merit high praise from their colleagues in evolutionary biology and history for assembling this exciting collection of papers. These will appeal to a readership in scientific and humanistic disciplines and provide stimulating topics for seminar discussions for instructors and university-level students. Some highly technical portions of the essays on microbiology, evolutionary processes, and the particular-circumstance model of scientific explanation place the book beyond the grasp of lay readers unfamiliar with current issues in evolutionary biology, history, and philosophy of science. The chapters have been carefully edited, but the writing styles vary, as is to be expected in any compendium of research papers. There is an author-subject index and a section with short biographies of each contributor. Bibliographies of cited references appear at the conclusion of each chapter. Graphic materials are minimal and judiciously selected. If a final word may be appended in order to properly define the character of this volume, it is to be found in the concluding statements of Slobodkin's paper, "Evolution of Scientific Theories": "Both the state of the world and the state of the science which describes it change with time. Science changes its focus, its organizational structure, and its conclusions as a consequence of social and political events. But also nature itself goes through historic and developmental changes from which emerge previously inconceivable phenomenological regularities, which can act as foci for scientific inquiry . . . The 'evolution of sciences' is not within the subject matter domain of biological evolution, but there are, nevertheless, patterns and regularities in how both social and natural sciences change. Understanding of these

patterns may aid our understanding as both participants in, and audience for, science" (pp. 254-55).

KENNETH A. R. KENNEDY
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HEINZ REINWALD. *Mythos und Methode: Zum Verhältnis von Wissenschaft, Kultur und Erkenntnis*. Munich: Wilhelm Fink. 1991. Pp. 539. DM 98.

Heinz Reinwald's book has a magisterial scope that has become rare in contemporary scholarship, for good or for ill. Reinwald proposes a sweeping reinterpretation of the nature of logic and its role in the rise of the West. He argues that logic has an unrecognized mythic character—hence the title—and treats the history of efforts to break free of the realm of myth by means of logic and scientific knowledge as a series of unwitting repetitions of the West's foundational myths. Although such a story would make for a book in itself, this one is also much more: a theory of culture; an intellectual history of modern scholarly traditions for studying myth; a historical explanation of the origins of three distinct regional cultures in modern Western and Central Europe; and a critique of what Reinwald considers to be the pretensions of science and Enlightenment. It is also admittedly, indeed proudly, an attempt to revive older, pre-1933 traditions in German scholarship.

The foundation of Reinwald's account is a theory of the cultural function of logic. Drawing on the work of Georges Bataille, he argues that all culture is a response to the existential "ur-angst" provoked by the experiences of death, sexuality, and want (p. 85). The order established by culture erects a "wall against nature" (p. 458), but in myth cultures also acknowledge that the human order is ultimately inseparable from nature and reconcile themselves with it. The creation of logic, too, is provoked by anxiety at the presence of the threatening, radically "other" and represents a ritual of self-purification, but, unlike healthier mythic culture, logic denies any implication in its other and pursues the dangerous chimera of pure, absolute knowledge.

The book's historical centerpiece is an account of the culture of Greek antiquity as governed by the mythic principle of *agon*, or ritualized struggle and reconciliation through sacrifice. Reinwald traces this agonistic pattern in fascinating detail, moving from athletic competition to the culture of politics, war, and the arts. But with the advent of Parmenides of Elea's demythologizing doctrine of absolute Being, Greek antiquity—and all of Western culture in its train—began a wrong turn. Parmenides inaugurated what later became Europe's tradition of denying its grounding in a threatening, heterogeneous nature. Whereas Plato and the Attic tragedians still attempted to re-create a harmonizing, reconciliatory logic, Aristotle took up Parmenides's exclusionary logic of non-contradiction and codified the forms of syllogistic reasoning that would become canonical in the West.

Reinwald goes on to argue that the medieval reception of Aristotelian logic laid down the path for Europe's transition to modernity. The recovery of Aristotle's works was mediated by Arabic cultures, which had introduced their own "expressive-activist" (pp. 355–56) moment into his doctrines. European logicians in turn developed this new emphasis in the direction of an even more radical, subject-generated philosophy. Meanwhile, medieval logic diverged into the three styles of thinking characteristic of Europe's regional cultures: Peter Abelard (and then René Descartes) established the deductive French style, descended from Dominican realism; William of Ockham (followed by Francis Bacon) forged the inductive "Anglo-Saxon" style, descended from Franciscan nominalism; and, finally, a third style emerged in German-speaking Central Europe, a mode of thinking that is analogical, hermeneutic, and able to accommodate contradiction. This third lineage stretches from the mysticism of Meister Johannes Eckhart to Gottfried Wilhelm Leibniz, Georg Wilhelm Friedrich Hegel, and beyond. Reinwald closes his historical exposition by arguing that this tradition, with its greater understanding for culture's inevitable implication in its other, is better poised than its Western counterparts to help overcome Europe's destructive, anti-mythic culture.

This book sits at a peculiar crossroads between two approaches to culture: one broadly speaking post-modern and the other an older, conservative tradition of German cultural criticism. Their point of intersection is the critique of rationalism. Reinwald is carefully ambiguous about his allegiances. He sometimes invokes the vocabulary of the postmodern Left, calling his work "a contribution to intercultural understanding free from domination" and his goal "enlightenment about enlightenment" (p. 20). One hears in this echoes of the left-wing variant of the argument developed by Max Horkheimer and Theodor Adorno in *Dialectic of Enlightenment* (1944), which, surprisingly, goes unmentioned. But at bottom, Reinwald restates the position of conservative *Lebensphilosophie*, with its critique of progress as fateful, mythic repetition and its unabashed assertion of Germany's cultural superiority.

Many historians will also find the book's method unsatisfying. Reinwald repeatedly insists on historically specific accounts of the function of logic. But his analyses of epochal crises are too general to be helpful, and he bypasses middle-range social and institutional factors to invoke the underlying constants of his philosophical anthropology. Ironically, despite his gestures toward the sociology of knowledge, he ends by reverting to an idealist account of the development of cultures in terms of patterns laid down by their great thinkers.

Despite such shortcomings, this book will make fascinating reading for those who would enjoy disentangling its ideological ambiguities, for cultural and intellectual historians concerned with particular seg-

ments of its argument, and for devotees of the venerable, if not always fashionable, genre of macro-history.

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FRANK FÜREDI. *Mythical Past, Elusive Future: History and Society in an Anxious Age*. Concord, Mass.: Pluto. 1992. Pp. ix, 310. \$21.95.

Frank Füredi's book addresses many of the debates that, having originated within academic disciplines, ended up in the public domain, serving to fuel the notorious "culture wars." The basic theme of the book is straightforward: the chaotic conditions of contemporary life in the West compel people to abandon orientations to the future and to turn to mythicized versions of the past in order to achieve closure or sustain a sense of identity in a threatening world. According to Füredi, the universally inclusive promises of liberal capitalism and (vulgar) Marxism have failed, enhancing the opportunities for hitherto excluded groups to make particularistic claims on the basis of gender, race, and ethnicity. The ideological adherents of the three contending factions, neoconservatives, Marxists (including critical theorists [pp. 253, 259]), and "neo historicists" (pp. 227, 231), then use these highly visible public debates to promote their own mythicized versions of the past. Politically, Left and Right may be significantly different, but in terms of this ideological use of history and the past they are not different at all (pp. 27, 227, 260). The author hopes that by illuminating this situation, and by offering a "non-ideological" approach to history, he can redirect the attention of people from the past to the future, for he believes that the current condition of uncertainty, crisis, and fear of social change (p. 13) is correctible on the basis of purposeful and reasoned human activity (pp. 77, 267–68).

Thus, Füredi develops two important arguments in his book. The first involves the repudiation of the persistent, ideologically partisan—particularly nationalist—manipulations of history, theory, and politics (pp. 85–87). The best way to explain what he means by this is to quote one of his (neoconservative) examples (p. 160): "as Communism collapses, the greatest ideological threat to western civilization now comes from within the West's own cultural institutions—the universities, the churches, the professions such as law and medicine, and above all the disintegrating family." Obviously, the author of this statement deliberately segregated government and business from any moral critique, although disinformation and public lying have become systemic and structural features of governance, while the sexualization of the environment and the breakdown of moral restraint, which must affect family life, stem more from the needs of capitalist enterprise than from any other source. This is not all that can be said

on the subject, certainly, but it is enough to make the point that the author of this quote did not specify the different sources of ideological threat as precisely as he could have.

The second argument involves the need to move beyond the impasse caused by the failure of the regulative constructs of the contending factions. In these terms, Füredi can be exasperating, for in his desire to explain his alternative to the debate he is either contradictory in his own claims or he fails otherwise to justify or defend them. He argues that the values of liberal capitalism are exhausted (pp. 88, 97, 133, 154, 189), suggesting a variety of dire consequences likely to follow from this (pp. 129–39, 162, 177, 245), even as he explains that nothing in history can be assumed as unproblematic or self-evident and that history cannot have a fixed character or essence (pp. 263, 265). If this is the case, then the values of liberal capitalism could as likely recover as expire from their exhaustion. Again, to lend a sense of urgency to his argument, Füredi must claim that everyday life in Western societies following the collapse of communism is more chaotic than ever before, a difficult proposition to defend. In attempting to defend it, however, the author uses the language of anxiety, conflict, crisis, exhaustion, fear, and unease more often than Sigmund Freud did in his most pessimistic work, and more obsessively than can possibly be justified. (In addition to repeated references to the language just noted, Füredi also uses the language of breakdown, catastrophe, decay, despair, deep-seated insecurities, fractured identity, intellectual incoherence, loss of confidence, loss of control, malaise, millennial doom, pervasive pessimism, profound crisis, profound disillusionment, profound doubt, stagnation of mind, and terminal decline.) I would not minimize the significance of current problems in Western societies, but I would not minimize the capacity of these societies to cope with them either. Then, too, in order to establish the viability of his “non-ideological” version of history and of future prospects (pp. 59, 70–71, 193), Füredi must offer a version of Marx that, in its exclusive, emphatic sense of history as contingent and open-ended, appears quite unreal. There was a “vulgar” (or to use his own term, a “stupid”) Marx whose published work promised or strongly implied that the future course of history was determined and inevitable, even if Marx did actually know better, and it was this version of Marx that inspired the revolutionary movements with which we are all familiar.

Finally, Füredi makes use of concepts of identity, reason, consciousness, and the transformation of consciousness (p. 259) as if they are unproblematic in their content. He also distinguishes rational from nonrational and irrational thought and action as if any thought and action does not harbor elements of all three at the same time, which is what makes life so problematic for people and social process open-ended and contingent (pp. 204–10). In any event, the

kind of knowledge of mental and social processes that might justify writing so facetiously on such complex subjects simply does not exist. These objections taken together raise serious questions about his sanitized version of Marx and his generous faith in human consciousness and reason.

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PETER BURKE. *History and Social Theory*. Ithaca: Cornell University Press. 1993. Pp. ix, 198. Cloth \$37.50, paper \$14.95.

There are neither theory-free facts nor theory-free historians, but not all historians have recognized this. Those who would like to see, think about, and use more creatively the theories imbedded in historical language and thought could scarcely have a more useful and genial guide than Peter Burke. This volume is a revision of Burke's *Sociology and History* (1980) so extensive as to constitute a new book.

Sociology and History also covered social anthropology; *History and Social Theory* similarly has a wider scope than its title indicates. Social theory includes cultural theory, and the book treats as well psychology, women's history, the “new historicism” in literary criticism, and even the rhetoric of historiography. Although Burke modestly apologizes for the fact that most of his examples come from his own field of expertise (early modern Europe), one of the charms of his book is the range of examples, which come from several parts of Africa, Indonesia, Brazil, China, India, Japan, Melanesia, Mexico, Pakistan, and Peru. The producers of social theory have almost all been Europeans, but the fact that these theories can illuminate the history of all these parts of the world shows they are not merely parochial generalizations.

Although this book has been brought thoroughly up to date, Burke notes that he has been careful not to become too up to date. Karl Marx and Max Weber are still the most frequently mentioned theorists. The heart of the book remains the relationship between history and social theory (as conceived by sociology and anthropology; economics figures largely as a source of “laws” that do not hold). This might suggest the sort of “social theory” that for many conjures up the vast systemic constructions of Auguste Comte, Marx, or Talcott Parsons, and it provokes a strong allergic reaction. These grand systems do make an appearance late in the book, but only after a discussion of the middle-range concepts that figure, explicitly or implicitly, in every significant historical account.

How, for example, do we think about social groups or types? Some historians eschew terms like “feudalism” or “puritanism” because they cannot be precisely defined (that is, shown always to exhibit a unique set of attributes). Burke points out that such terms seem

to be essential in historical discourse and need not be discarded simply because a definition of this sort is impossible. Thus, typical manors contain lords, vills, freemen, and courts. Although F. W. Maitland correctly pointed out that some manors lacked one or even more of these attributes, this does not render the concept unusable so long as all manors possess at least most of these attributes and each attribute is shared by most manors (pp. 32–33).

Burke goes on to treat an interlocking set of central concepts: social roles, gender, family and kinship, class, status and social mobility, "symbolic capital," reciprocity, patronage and corruption, power, center and periphery, hegemony and resistance, mentality and ideology, communication and reception, orality and textuality, and myth. The book could be used as a reference work, since the index would quickly inform the historian puzzled by "*habitus*" or "*mentalités*" where these are elucidated.

Burke's treatment of social class and status is exemplary. Census categories of occupations are static and cumbersome, whereas for Marx sometimes there were three classes, sometimes two, and sometimes it depended on whether there was class consciousness. Historians dissatisfied with Marxian terminology, especially those studying pre-industrial societies, have been drawn to a basically Weberian model dividing societies into estates or orders. Burke characteristically concludes with an irenic recommendation that historians use both concepts.

Burke acknowledges that such concepts are relatively easy to pilfer, whereas ideas like structure and function, culture, and the fictive element in historiography are "more dangerous." Yet the adventurous historian has much to learn from attempts to reconcile the importance of historical agency and the institutions within which action is conducted, or how to choose among various psychological theories. What is at stake in these theoretical choices becomes clear in Burke's final chapter, "Six Monographs in Search of a Theory."

So much is compressed into the 165 pages of text that omissions and miscues are inevitable, although these seem rare. More off-putting to some will be the relentless blandness of the text. Burke almost always states a theory, mentions the leading criticisms of it, and comes down somewhere in the middle. This minimizes the chance of misleading readers at the risk of boring them; but whereas this ubiquitous judiciousness gives the book the flavor of a textbook, it is a textbook that every beginning historian (and not a few of their elders) would profit from reading.

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ROBERT PAUL RESCH. *Althusser and the Renewal of Marxist Social Theory*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xi, 436. \$40.00.

Robert Paul Resch's weighty volume explicates the structural Marxism of the French philosopher Louis Althusser (1918–90) and other related theorists. Famously, in the preface to his *Contribution to the Critique of Political Economy* (1859), Karl Marx gave a thumbnail account of the materialist conception of history. Crystallizing views that he and Friedrich Engels had articulated in their unpublished *German Ideology* (written in 1845–46), Marx set forth a collection of concepts that, ever since, have been central to historical materialism. The theoretical project of the structural Marxists is centered on the tricky questions that arise when one begins to work with these concepts. What is the relation between the forces of production and the relations of production, which together make up the mode of production? What is the relation between mode of production (base) and superstructure? What is ideology and how does it function? What historical role does science play? How does historical causality operate?

Resch describes in considerable detail the answers that the structural Marxists gave to these questions. We might usefully see Resch as trying to do for these modernist successors to Marx what G. A. Cohen, in *Karl Marx's Theory of History: A Defense* (1978), did for Marx himself, although Resch lacks Cohen's considerable philosophical acumen. Resch offers informative accounts of important aspects not only of Althusser's work but also of the historical, philosophical, political, and aesthetic views of such Marxian thinkers as Nicos Poulantzas, Pierre Macherey, Étienne Balibar, Terry Eagleton, and Perry Anderson. There are also discussions of Gilles Deleuze and Michel Foucault.

In his "Introductory Conclusion" Resch interprets the economic, social, and political developments of the last fifteen years or so in terms of class struggle, manifested in the contradictions of both bureaucratic socialism and "Fordist" capitalism (pp. 1–32). He also attacks what he sees as the unfortunate pluralism, relativism, and individualism of a variety of perspectives opposed to structural Marxism. Many readers will find this opening section strident and off-putting. The arguments that Resch offers in support of his broad claims are inadequate: in essence, he simply assumes the correctness of the Marxian vision of society and history, tending to let vigorous assertion substitute for justification. In chapter 1 he attempts to distinguish the genuinely Marxian conception of "structural" causality from the other types, "transitive" and "expressive" causality (pp. 33–52). The distinction remains unclear to me, but this is probably Althusser's fault rather than Resch's, and the chapter does contain much interesting material for anyone who wants to think about historical materialism in general or about Althusser's work in particular. The chapters that follow focus on the mode of production (including a survey of some of the literature on the transition from feudalism to capitalism), science and philosophy, ideology, literature, and class struggle, political power, and the capitalist state.

Resch makes no attempt to explain the emergence of Althusserian theory or to link it to Althusser's personal history. Althusser the man is entirely absent from the book. To learn about Althusser's background, upbringing, education, and personal troubles (which culminated tragically in November 1980, when, in a fit of insanity, he strangled his wife), one must go to his posthumously published autobiographical writings (Louis Althusser, *L'Avenir dure longtemps, suivi de les faits: Autobiographies* [1992]; *Journal de captivité: Stalag XA, 1940–1945* [1992]) or to Yann Moulier Boutang's biography (*Louis Althusser: Une biographie*, vol. 1, *La Formation du mythe (1918–1956)* [1992]). There is also now an Althusser archive.

Although Althusser's strength lay in his deductive rigor rather than in any broad empirical grasp, he was often acute as a philosopher and as a historian of philosophy. An attempt by a historian to situate Althusser's intellectual work within the contexts of his life and times might well prove fruitful; certainly, in the wake of his death, at least some of the relevant source material is more readily available than before.

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PETER RIESENBERG. *Citizenship in the Western Tradition: Plato to Rousseau*. Chapel Hill: University of North Carolina Press. 1992. Pp. xxiv, 324. \$42.50.

The past is a foreign country, as we know, but some institutions seem to place us on a familiar footing even with people of a remote antiquity. "Citizenship" apparently brings us city dwellers together in a common experience that shows continuity over some two and a half millennia and more. Peter Riesenberg traces the political, social, and intellectual history of this social and institutional creation from the Greek *polis* and Roman *civitas*, through residual medieval forms and Renaissance revivals, to its eclipse under the monarchies of the Old Regime and its reemergence in the cosmopolitan agglomerations that succeeded the French Revolution. The book is concise and comprehensive, modest and scholarly, thoughtful and judgmental, and based on a lifetime's research in the late-medieval stages of civic history.

For Riesenberg there were two citizenships. "The first lasted from the time of the Greek city-state until the French Revolution; the second has been in existence since then," he argues; "it was small-scaled, culturally monolithic, hierarchical and discriminatory—also moral, idealistic, spiritual, active, participatory, communitarian, and even heroic." The second citizenship, drained of civic virtue, was based on "the slack association of the many" and situated "in a large territorial state whose size makes face-to-face politics impossible." In this civic twilight citizens were "submerged into the general will and lost that individuality which Aristotle, Augustine, and Machiavelli valued so highly, each in his own way" (pp. xviii–xix).

From this perspective Riesenberg tells the story of a world we have lost, to our material gain but moral deprivation. City life of course preceded the experience of the Greeks, but Riesenberg begins with them because they were the first to reflect on political organization. Spartan and Athenian citizenship, which was reinforced by militaristic, religious, intellectual, and pederastic bonds as well as the devices of democratic government, represented a moral and "communitarian" phenomenon. This short-lived achievement survived as a philosophical and cultural ideal that, in its Hellenistic form, envisioned a universal *humanitas*, a concept that was taken up and realized in more practical fashion by the Romans. The theory of citizenship—the laws defining the *civis*—was given its classical formulation in Roman law and in this form was passed on to western Europe.

The early medieval period brought a "new localism" in which religious bonds counted more than civic responsibilities. A revival of commerce and consequent civic rivalries, however, stirred memories of ancient citizenship, created the conditions for a renewed study of ancient law and philosophy, and produced a new social form and accompanying ideology, especially in Italy. In return for support and protection, citizenship demanded "money, military service, conformity to its culture and law, and veneration of its saint" (p. 135), and the result was a "new civic consciousness." To this topic Riesenberg brings a special expertise acquired over many years of studying the sources of Italian legal history, especially the extraordinary *consilia* literature, in which the grades and terminology of late-medieval citizenship were worked out in great detail. What Riesenberg sees in particular is the progressive "extraction of the individual from his corporation," the recognition of individual rights, and finally the development of a "common law of citizenship" (p. 174).

This was the larger background of the more particularized movement of "civic humanism," whose center of gravity had shifted forward in time and whose configurations have been the focus of so much recent controversy. Having been immersed in technical legal literature rather than political thought of a Machiavellian order, Riesenberg has qualifications to bring to the work both of Hans Baron and of J. G. A. Pocock (a former colleague of Riesenberg's whose "power of imagination and persuasion" [p. 189] he still has misgivings about). What Riesenberg has to add to this historico-ideological debate is a more concrete context, the practical experience, judgments, and writings of lawyers, who, he remarks, "systematically . . . raised every question" about questions of citizenship. "All the great authors discussed citizenship in some respect," Riesenberg writes (p. 175); but for him these "great authors" include not only Thomas, Dante, Petrarch, and Salutati but also Bartolus and Baldus; and for him civic humanism had a foundation not only in the rhetoric of humanists but also in the statute law of the communes, which

was "supported by centuries of discriminating commentary on the *Corpus Iuris Civilis*."

From his medievalist standpoint Riesenbergs sees citizenship subordinated to the institutions of absolute monarchy from 1500 to 1800: republican ideals being replaced by princely ones and the status of citizen by that of subject. In this part of the book, however, Riesenbergs no longer draws on the writings of the jurists on the level of the *consilia* literature but is content for the most part with the more familiar texts in the canon of political theory from Machiavelli and the Monarchomachs to Rousseau and the revolutionaries. In this mainstream of political thought, citizenship is overshadowed by the larger concerns of statecraft and problems not of the *civis* but rather of the subject. Except perhaps in his educational writings, Locke stands outside "the mainline tradition of republican citizenship" (p. 250); and even James Harrington, despite the arguments of Pocock, does not appear as an impressive champion of the civic tradition or defender of the rights of citizenship. In general, the idea of citizenship did not extricate itself from its attachments with the ancient city-state until the French Revolution created its own republic and accompanying ideal of the *citoyen* (and *citoyenne*), and the basis of the sad, post-civic, "second citizenship" of Riesenbergs's story.

For Riesenbergs, citizenship is "an ambiguous institution," and so, finally, is his analysis. He adopts a moral viewpoint that celebrates the civic ideal even though it was strikingly elitist in its periods of glory and even though the real history of the communes, despite their liberties, was largely, as he acknowledges, "a chronicle of hard times" (p. 120). Yet the cities of a later period are criticized for having "fallen to merchant values" and having produced a passive citizenry that, "beer in hand," watches television and stays away from the polls (p. xx), and that perhaps does not even care for stories about the Greek agora, the Roman forum, or the public space of the Renaissance city-state. To this extent we may agree with Riesenbergs that his work is perhaps "a study in the persistence of a delusion or myth" (p. xxiii). But it is a persistent delusion and a noble myth, and it is useful to have this large-scale review even if (or especially because) it leans more toward the issues of political thought than the realities of city life.

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MASSIMO LIVI-BACCI. *A Concise History of World Population*. Translated by CARL IPSEN. Cambridge, Mass.: Blackwell. 1992. Pp. xvi, 220.

The title of Massimo Livi-Bacci's work is somewhat misleading, since the book does not pretend to offer a chronological history of world population, however concise. Rather, it discusses major trends in the history of human population, concentrating on theo-

retical and empirical relationships between economic development and population growth, from pre-neolithic times to the present.

The five chapters are arranged chronologically. In chapter 1, the author offers an interesting introduction to the biological dimensions of human reproduction. He identifies the human species as one characterized by a survival strategy known to biologists as "K-type"—typical of medium and large mammals. This survival strategy rests on inhabiting "relatively stable environments," successful competition with predators, "considerable investment of time and energy for the raising of offspring" (p. 3), and a growing ability to control the environment. Livi-Bacci probes more deeply into the biology of population growth by discussing determinants of the length of birth intervals, introduces basic concepts of the life table, and sketches out the broad stages of human population growth.

Chapter 2 introduces the theme of "choice and constraint," the two factors Livi-Bacci identifies as main determinants of population growth trends. Constraints come from the environment, from spatial restrictions on settlement, and, most importantly, from food resources. "Choices" here refers to large-scale social practices that limit access to reproduction, such as delayed marriage or high levels of permanent celibacy. Livi-Bacci's constraints and choices closely follow Thomas Malthus's distinction between positive and preventive checks on population growth, with one major exception. While noting, as does Malthus, that epidemic diseases have acted as a powerful force in controlling population growth, Livi-Bacci, like many historical demographers, considers epidemics as a force exogenous to the sociodemographic system, which is founded on the relationship between population and resources (p. 48). In the rest of the chapter, the author presents some well-known examples of catastrophic demographic decline and of rapid demographic growth, showing the wide range of variations in growth that have occurred in the medium term of human population history. Here and throughout the book, Livi-Bacci is much more interested in "constraints" than "choices."

In chapter 3, which concerns demographic growth in agricultural societies, Livi-Bacci uses the concept of diminishing returns to address the theme that is clearly of greatest interest to him: the relationship between economic growth and demographic growth. He presents a wide variety of historical and current examples to examine both sides of the perennial debate between those who view population growth as a boon to development and those who see it as a force mitigating development. Livi-Bacci concludes that demographic growth may be a positive force for development in the long term, but that its medium-term (a "few decades" to a "few generations") effects may counter economic growth (p. 99).

Chapters 4 and 5 address an oft-made comparison between the history of the "demographic transition"

in the West beginning in the nineteenth century and its progress in the less-developed countries since the end of World War II. Livi-Bacci addresses major ways that the two situations differ. He underlines the importance of relatively low rates of reproduction in the West before the decline in death and birth rates began, the relative ease and importance of mass emigration from Europe in the nineteenth century, and the rapidity of mortality decline in many parts of the Third World. Chapter 5 contains a valuable discussion of differences among less-developed countries in the relative progress of their fertility declines, and, although he attributes much of the variation to differences in per capita income, Livi-Bacci also argues for the importance of government policy advocating birth control.

Livi-Bacci's book is synthetic rather than interpretive. It provides a useful introduction to broad, substantive issues in the history of human population at a high level of aggregation. The historian might fault the author for his neglect of cultural dimensions of demographic "choices" that many demographers are now rediscovering as central to explaining demographic processes. This neglect is rather odd given the fact that it is through culture that human populations have successfully implemented the "K-type" survival strategy from which the author so usefully begins his discussion.

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G. ROBINA QUALE. *Families in Context: A World History of Population*. (Contributions to the Study of World History, number 35.) New York: Greenwood. 1992. Pp. xiv, 466. \$59.95.

At the end of chapter 1 of this book, G. Robina Quale identifies its three major themes: how households have tried to attain the dependency ratio (three productive persons to two dependents) that she defines as optimal; how economic development and the nuclear family have spread from their origin in Western Europe to other parts of the world; and how the consequent environmental degradation may be overcome. These three themes are explored in ten chapters divided into sections dealing with broad historical periods: Neolithic; 3500 B.C. to A.D. 1500; and A.D. 1500 to the present. The volume concludes with twenty-nine bibliographies.

The strongest aspect of this book is its wealth, indeed surfeit, of facts. Facts require organization, however, and there Quale is much less successful. For example, one might ask how the three major themes are treated in the concluding chapter. Its title indicates that it deals with the transition from world cities to the world as a global village. It begins with the observation that nineteenth-century world's fairs taught ordinary people everywhere that human life is organized around the domestic sphere and that

"global trade, national governments, international diplomacy, and arts, letters, and sciences came into being to make life better for individual, family, and community" (p. 378). The chapter goes on to identify the five goals of family life; to argue that the fertility transition resulted from constricted opportunities for migration; and to suggest that "nontraumatic gradual decrease [in population] may be a genuinely sensible evolutionary response to human beings' current over-use of global resources" (p. 378). The second of the three themes, the spread of the nuclear family, appears in this concluding chapter, but environmental degradation is mentioned only in passing, and optimal dependency ratio does not appear at all.

This lack of organization makes it difficult to determine this book's audience. Scholars new to the topic might look, in a book subtitled "A World History of Population," for an analysis employing the broad material of population history—fertility, mortality, migration, age structure, marriage, and household formation—and organizing it around the central theoretical concepts of the field. They might expect to find John Bongaarts's model of the proximate determinants of fertility, Abdel R. Omran's discussion of the epidemiologic transition, or John Hajnal's contrast between family formation systems in historical northwest Europe and elsewhere. All the elements of these concepts appear innumerable times, but they are never linked into a coherent argument. For example, the major event of world population history, the demographic transition, does not even appear in the index, much less as a central organizing concept.

Although the volume is loaded with facts, however, the student looking to it for specific information will have great difficulty. The book has no subheadings below the level of the chapter title, and within chapters there is no consistent organization. For example, the population history of Japan (the topic with which I am most familiar) appears in a chapter that also touches on ten other countries. That section begins by speculating that there was a fertility transition in the eighteenth century and then moves on to the role of outcasts in the 1600s and the 1980s. The immediately preceding section, on Korea, begins with the thirteenth-century transition from matrilineality to patri-lineality and then mentions life expectancy at birth (which Quale neologistically terms "birth expectancy") in the 1930s.

It is also very unusual to find a book about population that contains almost no graphs or tables. Series of figures that should appear in a graph or table, where they would be both clear and open to further analysis, are instead presented as a narrative string. "Extended households dropped from 36.5% in 1955 to 29.2% in 1965, 22.2% in 1975, and 19.7% in 1985" (p. 373). This style of presentation both obscures the relationships among numbers and convinces those who are uncomfortable with them that they are inherently tedious. Finally, the bibliographies bear no

direct relationship to the contents of the chapters. The one on Japan omits two important sources (T. C. Smith, *Nakahara: Family Farming and Population in a Japanese Village, 1717-1830* [1977], and Irene Taeuber, *The Population of Japan* [1958]) and relies excessively on conference papers. It would be imposing a penance to require anyone to read these acres of small gray type for a class.

It is unfortunate that an author with such great skill and talent at accumulating information could not put it to better use. I cannot recommend this book.

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AZAR GAT. *The Development of Military Thought: The Nineteenth Century*. New York: Clarendon Press of Oxford University Press. 1992. Pp. viii, 273. \$69.00.

In this book Azar Gat continues to pursue a major theme developed in his *The Origins of Military Thought* (1989): the Enlightenment created an intellectual climate that was at once rational, critical, and practical. Along with the Enlightenment came the scientific revolution, which extended methods of critical and empirical enquiry to the military arts. Gat's present work examines the further intellectual development of military thought through the nineteenth century.

Given the historical sweep of the period, Gat must necessarily limit his range of inquiry to present a more comprehensive survey of his subject. He accomplishes this in fine fashion by focusing primarily on the French and German military thinkers of the period. Relatively less attention is given to the British and American thinkers. A notable exception, however, concerns Gat's summary of the work of naval theorists Alfred Thayer Mahan and Julian Corbett. The two provide a contrast in thinking, well developed by Gat, already evident among the preeminent military thinkers of the age. Corbett, an ardent disciple of the military sage Carl von Clausewitz, emphasized the importance of naval actions working in support of operations on land. This maritime strategy was more embracing than that of Mahan. An astute student of A. H. Jomini, Mahan stressed the independent role of the navy in its search for a climactic battle at sea. Gat explains the contrasting views between Corbett and Mahan as a consequence of the divergence of views regarding war on land expressed in the writings of Clausewitz and Jomini.

The cause of this divergence of thought Gat leaves partially unstated. Yet this notion of divergent thinking could easily constitute another unifying theme throughout both of his books had it been more fully developed. Gat seems to equate military thought with military theory. This is an unfortunate generalization, but perhaps necessary given the comprehensive scope of his project. In any case one can discern a rather loose intellectual division of labor among mil-

itary thinkers defined by the relationship among theory, doctrine, and practice. There is, however, no guild-like association to the three dimensions of military thought. Instead, thinkers migrate freely among all three. As military thinkers mature intellectually they tend to settle in one of the three camps. Mahan, for instance, had very little interest in the practice of his craft. He struggled mightily and unsuccessfully to avoid assuming command of a cruiser, the *USS Chicago*. Clausewitz wrote somewhat on the practical aspects of war, but his fame today rests with the theoretical treatise *On War* (1832). Jomini, like Mahan, was a doctrinaire. The appellation is unfortunately pejorative but serves as a useful identifier. Theoretical system builders like Clausewitz and Corbett were more interested in finding out how war works by identifying the causal connections and linkages among the many moving parts. Where Clausewitz and Corbett developed "blueprints" of war, Jomini and Mahan wrote "users' manuals" for practitioners, and still others were left to put it all together in practice. Considering the instrumental nature of war, one can see the importance of all three dimensions of military thought.

From the standpoint of form, readers will find few distractions, although I experienced a vague sense of whiplash as the author careened back and forth across several score years of history in the space of a few pages (pp. 21-22). On a more substantive level Gat seems too wedded to the view that the Napoleonic wars mark a revolution in warfare. There is a growing body of scholarship suggesting that the so-called Napoleonic revolution was in fact the culmination of a much longer military evolution evident throughout the entire preindustrial period. Arther Ferrill (*The Origins of War* [1985]) was among the first to raise this issue. The emerging revisionist view sees the Industrial Revolution as the chief causal agent in the transformation of warfare; thus, the origins of modern war are to be found in the American Civil War rather than in the wars of Napoleon. The collapse of the Napoleonic paradigm led to the eclipse of classical strategy and the emergence of what we now term operational art. In view of Gat's adherence to the older view it is not surprising that one of the foremost theorists concerned with operational art, Sigismund von Schlichting, receives short shrift in the discussion of German theorists.

In a more positive vein, however, Gat allows us to peer over the shoulders of the nineteenth-century thinkers and watch their ideas unfold as though they were in the very act of putting thought to paper. Taken together with his earlier work, Gat's study will likely become the standard text on military thought in civilian as well as military schools for some time to come.

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JOHN S. HALLER, JR. *Farmcarts to Fords: A History of the Military Ambulance, 1790–1925*. (Medical Humanities Series.) Carbondale: Southern Illinois University Press. 1992. Pp. xii, 269.

This work deals with two interlocking aspects of modern military medicine. First and foremost, as indicated by the title, it examines the changing modes of transportation utilized by successive armies in history to evacuate the wounded from the battlefield. In addition, however, it explores in some detail the army hospitals and civilian backup facilities available to those brought in by the evacuation systems.

In the process, John S. Haller, Jr., clarifies for modern readers the historically changing uses of the term “ambulance.” In its original sense, the ambulance was one or more easily moved military hospitals placed near the front lines, a system developed by Napoleon’s surgeon, Dominique-Jean Larrey, and long used in European armies. Only later in the nineteenth century, and particularly in British and American usage, did the term gradually also come to signify the various wheeled vehicles employed to evacuate the wounded, either to semimobile hospitals of the Continental type or to larger, better equipped, and more permanent facilities at a distance from the fighting. Although Haller has given somewhat greater attention to developments of such services and facilities in the British and American armies, he also extensively examines the important French and German experience and more briefly covers that of other countries. His account is chronologically arranged.

The work is compact and well documented. It includes abundant factual information on the origins, construction, and use of such evacuation modes as stretchers, animal litters, wagons, bicycles, motor vehicles, trains, ships, and airplanes, concrete testimony to the shifting technology of battlefield humanitarianism. At the same time, Haller offers important insights into certain other factors that have affected battlefield medicine. Notable among them is the destructive effect of modern war weapons, not only in producing ever more serious wounds but also in straining and endangering the evacuation processes, and, conversely, the steadily growing capacity of military medical personnel over this period to provide more effective treatment of the wounded.

Among my few complaints with this volume is the author’s occasional use of the words “England” and “English” where “Great Britain” and “British” are clearly indicated. His lists of ambulance names and specifications provide greater wealth of detail than most readers will need. More substantively, his preoccupation with wars of the major Western powers allows for little exploration of the practices of evacuating the war-wounded among less-developed societies, guerrilla fighters, or isolated tribes.

Haller has, however, produced an original, broad-ranging, and authoritative study. Effectively orga-

nized, well written, and free of jargon, it includes much that will interest general historians as well as historians of warfare, medicine, and technology. As comparative history, it is both innovative and thought provoking.

JAMES H. CASSEDY
National Library of Medicine

RONALD L. NUMBERS. *The Creationists*. New York: Alfred A. Knopf. 1992. Pp. xvii, 458. \$27.50.

This study is Ronald L. Numbers’s account of the birth and growth of scientific creationism. Winner of the Albert C. Outler Prize in Ecumenical Church History of the American Society of Church History, the book argues for the formative role of the little-known Seventh-day Adventist George McCready Price and his textbook *The New Geology* (1923). It argues as much and more for the catalytic role of John C. Whitcomb, Jr., and Henry M. Morris, with their work *The Genesis Flood* (1961), written under the banner of belief in the inerrancy of the Bible. Published in an opportune cultural climate, *The Genesis Flood* read the biblical Book of Genesis according to the flood-geology interpretation that came to characterize the stance of creation science. Flood geology taught that life on earth was relatively “young,” emerging in a period between 4000 and 8000 B.C., with fossil layers formed after the universal flood that the biblical Noah survived. This theory, which in some forms admitted the existence of a lifeless earth in the time before the Edenic creation, was distinguished from older interpretations supported by conservative Protestants. The day-age theory (that the “days” of Genesis were lengthy ages) and the gap or “ruin-and-restoration” theory (that one or more cataclysmic events occurred between the original creation of matter and life and a much subsequent creation of Adam and Eve) were significantly different from flood geology. It took religious conservatives a while to recognize the difference.

Numbers is concerned with the story of how flood geology—as a theory an unlikely candidate for widespread epistemological acceptance—came from (seemingly) nowhere and garnered fundamentalist support. Price had constructed the theory as an elaboration of Adventist prophet Ellen G. White’s Noachian cosmogony, in which a world-wide flood was responsible for burying the fossil record and for reshaping the surface of the earth. And as Numbers’s construction of the tale reveals, it was a combination of popular theological leaders (Price, Whitcomb, Morris, and others) and a slowly building network effect that vaulted flood-geology creationism to success. Indeed, in the fifty years after Charles Darwin published *On the Origin of Species* (1859), virtually all Protestants accepted the antiquity of the earth and the progressive building of the fossil record. Among those who came to be known as fundamentalists, a

full-blown attack on evolution did not occur until the 1920s, and even then not in terms that supported flood geology. Thus, creation science is a phenomenon of the late twentieth century, a movement that grew in the wake of *The Genesis Flood*.

As this saga of creationism unfolds, Numbers unwinds it with energy and engagement in a tour-de-force style that for him is characteristic. His is a work of thick description, and the book is filled with names and narratives, with carefully crafted reportage of relationships and interactions among the flood-geology faithful. As a narrowly focused rehearsal of the persons and organizations that culminated in creation science, the book succeeds. It is meticulously and exhaustively researched, lucidly written, mostly fair in its portrayal of creationist leaders and rank and file, and attentive to transatlantic, and even worldwide, aspects of the tale.

Yet outside the immediate focus of the Numbers chronicle, one wishes for a more contextually driven account and a more self-conscious reading. Numbers notices, for example, that, within contemporary creation science, folk science is linked to folk religion. But much more needs to be done to unpack the class and cultural backgrounds that this observation suggests. And more attention needs to be paid to why people in our own time turned with such enthusiasm to flood geology. In the symbolic universe of American fundamentalism, what series of concerns did it signal and sum up? If Numbers answers only perfunctorily, this is in part because—in an ironic parallel to his subjects—a kind of (Baconian?) positivism informs his interpretation. He seems, in short, oblivious to the challenge of a relativistic postmodernism for historiography and, in general, not overly interested in issues of epistemology.

Still, Numbers has written a definitive work. It will be hard for any other study of creation science to match his mark, and his book should prove a vital resource for at least a decade.

CATHERINE L. ALBANESE
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JULIE WOSK. *Breaking Frame: Technology and the Visual Arts in the Nineteenth Century*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. xiii, 267.

Julie Wosk's title is a play on words. In his book, *Frame Analysis: An Essay on the Organization of Experience* (1974), the sociologist Erving Goffman used the phrase "breaking frame" to denote (in art historian Wosk's words) the "feeling of disorientation when . . . the basic frameworks of understanding used to make sense out of events no longer apply" (p. 3). At the formal opening of the Liverpool and Manchester Railway in 1830, for example, a dignitary became "alarmed and agitated." He was struck by the locomotive *Rocket* and later died from his injuries. Rail-

road travel, Wosk writes, "introduced a new world" to which traditional modes of cognition were inapplicable, and this experience of breaking frame "became an important new theme in nineteenth-century art, illuminating the century's undercurrents of uncertainty and fear" (p. 4). Along with this sort of imagery, however, there was a parallel artistic stream depicting a different reality, a vision of reconciliation and coherence. Wosk rounds out her play on words by introducing the reader to a machine called a "breaking frame," wherein short "slivers" of cotton were integrated into continuous strands for spinning. Hence her theme: "By visually clarifying the century's diverse cultural responses to technology, nineteenth-century artists were in a sense creating their own version of a breaking frame—providing a means to integrate slivers of experience in a disruptive, disjunctive industrial era" (p. 6).

Wosk's narrative and analysis is strongest in the first half of her text: her introduction and the initial two chapters, titled "The Traumas of Transport" and "Art, Technology, and the Human Image." She elaborates on paintings such as Philip James de Loutherbourg's lurid "Coalbrookdale by Night" (1801), evocative of Blake's indictment of England's "satanic mills," and George Inness's "The Lackawanna Valley" (1855), with its idealized integration of "the machine in the garden," in Leo Marx's classic metaphor. Although graphic artists working for American, English, and even French periodicals apparently had an eager audience for scenes of catastrophe, Wosk notes that American painters "tended to shy away from showing the more traumatizing effects of technological change" (p. 15). In her opening chapters, Wosk's address to a dialectical "fracturing and integration" is both subtle and forceful. She is less successful when she extends her analysis to the aesthetics of industrial design and volume-produced consumer goods. She has interesting things to say about electroplated tableware and cast-iron building façades, but her narrative tends to be repetitive, pitting the views of people who celebrated democratization in the arts, particularly Americans, against the predictable protests of John Ruskin, Charles Eastlake, and William Morris. She nevertheless closes strongly with a chapter on the application of neoclassical motifs to machinery, specifically the "architecturalizing" of stationary steam engines. Wosk suggests that this practice had diverse roots, but not the least important was a desire to construct a familiar "frame" in order to harmonize an "association with danger" (p. 200).

Abundantly illustrated and compellingly argued for the most part, Wosk's study stands as a valuable complement to the work of writers such as Leo Marx, John Kouwenhoven, Herwin Schaefer, and Barbara Novak in explicating the relationship of art and technology in the nineteenth century.

ROBERT C. POST
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MARGARET CANOVAN. *Hannah Arendt: A Reinterpretation of Her Political Thought*. New York: Cambridge University Press. 1992. Pp. ix, 298. \$54.95.

JEFFREY C. ISAAC. *Arendt, Camus, and Modern Rebellion*. New Haven: Yale University Press. 1992. Pp. xi, 320. \$30.00.

Margaret Canovan's well-crafted reinterpretation of Hannah Arendt's published work uses the latter's unpublished manuscripts and correspondence to demonstrate persuasively the continuity and coherence of her thought and the ongoing significance of the problem of "totalitarianism" throughout her career. Jeffrey C. Isaac's comparative study of Arendt and Albert Camus defends them as exemplary humanists and harbingers of "a postmodern theory of democracy" (p. 229) able to avoid the moral dead end of postmodern "anti-foundationalism" without slipping into the proto-totalitarian "foundationalism" of Marxist social theory. Whereas Canovan favorably associates Arendt's commitment to political plurality with the republican elitism of Montesquieu and Tocqueville (and, more implausibly, with the libertarian capitalism of F. A. Hayek and Ayn Rand), Isaac wishes to emphasize the importance of Arendt and Camus as avatars of postmodern dissidence and seekers of a democratic "third way" between capitalism and communism.

Canovan and Isaac rely a great deal on the explanatory power of "totalitarianism," a concept defined by Arendt and Camus on the basis of certain similarities of fascist and communist regimes. Totalitarianism involves a fanatical belief in an ideology that purports to explain the process of history in terms of an irresistible and progressive determinism; a messianic, yet woefully indeterminate, vision of the final outcome of this process; and, finally, an ominously authoritarian, unlimited, and ruthlessly instrumental attitude toward the means by which the final solution is to be implemented. The origins of totalitarianism are rooted in the murky confluence of human hubris and mass society. Human pride is responsible for the modern illusion that the "human condition" can be transcended by science and technology, an illusion that is historically realized as a dehumanizing triumph of instrumental reason over human freedom. Mass society stems from the modern attempt to emancipate the poor by industrialization, an attempt that has redefined politics in terms of economic expansionism and elevated the baser needs of consumption over the higher needs for citizenship. Arendt and Camus recognize capitalism as fundamental to the "modernization" process and the appeal of fascism and communism, but it is a capitalism driven more by political expediency and moral callousness than by private ownership of the means of production, commodification of labor power, and market competition. The economic crises, social conflicts, and international tensions that actually produced Adolf Hitler and Joseph Stalin are dissolved

into the general pattern of moral, political, and philosophical decline that has beset Western civilization since the Greeks.

Canovan and Isaac rightly distinguish the views of Arendt and Camus from pro-American Cold War liberals, but neither seems aware of how indebted their conception of totalitarianism is to European existentialism. For existentialists such as Arendt and Camus real history gives way to a Manichean struggle between "authenticity" (typically the provenance of a courageous and enlightened minority) and "self-deception" (usually identified with the cowardly and credulous masses). Totalitarianism is a state of willed self-deception, an evasion of personal freedom and moral responsibility in return for a false and morally debased sense of identity, empowerment, and security. The "bad faith" of totalitarianism is contrasted to the authentic moral position of humanism, a tragic awareness that, in an absurd world, we are each absolutely free and totally responsible beings morally obligated to recognize and defend the freedom of our fellow sufferers. Existentialism, in other words, deduces political good and evil from an ontology of the "human condition" without much concern for the intervention of historical processes or social structures. The opposition between "totalitarianism" and "humanism" is hopelessly idealist, and neither Canovan nor Isaac are able to move beyond the psychological impact of the horrors of the twentieth century in order to confront this fact. This said, the discussion of the efforts of Arendt and Camus to embody the existential humanist ideal in participatory democratic form constitutes the most stimulating sections of both books. Canovan, in particular, does a marvelous job weaving the many threads of Arendt's thinking on the problem of politics into whole cloth. Isaac, by placing Arendt and Camus in comparative perspective, makes a valuable contribution to an often ignored chapter in the history of the non-Marxist Left.

Canovan and Isaac are deeply sympathetic to the arguments of Arendt and Camus regarding the importance of political activity. Arendt and Camus view politics as an irreducible aspect of human existence inherent in the differences between people and in the complexities of social life. Condemning "totalitarian" and "modernist" attempts to eliminate politics, both oppose mass politics and bureaucratic administration in the name of participatory and decentralized democracy organized, in the case of Arendt, on the basis of local citizen councils. Arendt and Camus are each sharply critical of liberal, representative democracy because it is oligarchical and because it is based on the passive participation of citizens in political processes controlled at every level by elites. Arendt, however, does not mean that politics can or should be a mass activity, only that it should be open, regardless of occupation, to that minority of citizens for whom freedom is as essential as life itself.

Politics is the essence of human freedom and the fundamental right of citizenship for Arendt and

Camus, but neither is overly exercised by the social and economic preconditions for political participation. Arendt, Isaac points out, seeks to establish political freedom on the basis of an arbitrary (and, I would add, elitist) distinction between "politics" and "society" (the former being the truly human realm of "action" taken publicly and by agreement with other citizens, the latter being the less-than-human realm of "labor," required by biological necessity, "work," required to master nature, and "economy," the technical administration of routine activities). Camus rightly rejects such dualism and boldly calls for "a new social contract" based on the inseparability of political and economic reconstruction. Unfortunately, neither Camus nor Isaac gives an indication of what such a contract might entail or what forms of economic reconstruction might be necessary to support a truly participatory democracy.

Here we approach the conservative inner limits of "rebellious politics" that are implicitly approved by Canovan and stubbornly ignored by Isaac. Whereas neither Arendt nor Camus reject political violence as a means of change, both are deeply fearful of revolutionary projects aimed at total transformation. To such a "revolution" of excess Camus counterpoises his famous concept of moderate "rebellion": a determined pursuit of reform inhibited by a sense of limits, an awareness of the unpredictability of outcomes, and a sense of moral responsibility for the violence committed in the name of progressive change. Viewed as abstract, moral principles, there is hardly anything to quarrel with here. When viewed, however, in light of their egregious shortcomings as historians and social theorists—shortcomings honestly, if somewhat incongruously, elaborated by Isaac himself—the opposition of Arendt and Camus to revolution raises serious questions. What if the causes of social conflict are structural and fundamental rather than contingent and negotiable? How are we to distinguish what is actually possible from what is hopelessly utopian? When does fear of possible outcomes become a justification for doing nothing? At what point does a defense of human plurality become toleration of existing social inequalities? As Isaac's chapter on Arendt's views on the question of Palestine and Camus's writings on the Algerian revolt inadvertently reveal, neither Arendt nor Camus have any answers to these questions; more distressing perhaps is the fact that Canovan and Isaac do not even raise them.

Canovan's exposition of Arendt's search for a political framework based on human plurality is a fine piece of critical exegesis, while Isaac's discussion of the "rebellious politics" of Arendt and Camus, despite its unctuous anti-Marxism and postmodern posturing, is a stinging critique of any radical social theory that confuses social scientific knowledge with either political omniscience or ideological hopes and fears. Neither work, however, is able to evade, much less transcend, the charge that the political and moral vision of Arendt and Camus is flawed, perhaps fatally

so, by an evasion of the real history and logic of capitalist development. Perhaps, in order to avoid slipping into self-deception themselves, those who do not wish to talk about economic determination and class struggle had best be silent about equality and democracy.

ROBERT PAUL RESCH
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PETER DUIGNAN and L. H. GANN. *The Rebirth of the West: The Americanization of the Democratic World, 1945–1958*. Cambridge, Mass.: Blackwell. 1992. Pp. xii, 733. \$69.95.

Reading this book is like attending a lecture by an extremely knowledgeable, highly opinionated authority who is amusing or irritating (depending on one's point of view), but always interesting. Peter Duignan and L. H. Gann have two basic themes and one basic purpose. Their themes are the recovery of the democratic West following the destruction of World War II and the special role of the United States in that recovery. Their purpose is to counteract what they see as a tendency of scholars and students to ignore or be ignorant of the trials and successes of the postwar years and hence to take West European stability for granted. Here then is a big subject treated in a big book that deserves serious attention, both for itself and for the questions that can be raised about such works. They are, inevitably, works of synthesis. What should such a work accomplish?

If the purpose of a work of synthesis is to survey current and relevant literature and set down the findings of scholarship in detail, then this book ranks high. Duignan and Gann's account of the postwar recovery in the democratic West is immensely detailed, spiced with humorous asides, given typographic variety by boxed anecdotes and sketches, and punctuated with solid statistical data. The sources alone constitute a treasure trove from which graduate students will be drawing for years to come. The organization of the book, although it makes for a certain amount of repetition, facilitates assignment by section and assures that students have the necessary background information for each topic. In the category of synthesis-as-survey, this book ranks near the top.

Suppose, however, that the purpose of a work of synthesis is not simply to survey the scattered strands of scholarship but to gather them up and weave them into a connected and persuasive argument. Here the book would not rate as high. To a certain extent Duignan and Gann are the victims of their own breadth of knowledge and firm stance on right and wrong. Their staggering breadth of knowledge makes it difficult for them to select just the salient fact or the telling point. And their conviction of what is right means that sometimes the advancement of

knowledge is subordinated to the advancement of their cause.

This tendency is most evident in the sections dealing with the Cold War, where Duignan and Gann replay the scholarly battles of that period with all the vigor and partisanship of new recruits instead of seasoned veterans. In the discussion of Western intervention in the Greek civil war, for example, they proclaim the outcome as "an unmixed blessing for Greece" (p. 314). Anyone is entitled to an opinion, but in a scholarly work opinion should rest on more evidence than is offered here.

Their discussion of the McCarthy era suffers from the same penchant for argument by assertion, and their references to the Central Intelligence Agency (pp. 430–32) could, with profit, have been left out of the book. They do not have to agree with the arguments of Richard H. Immerman (*The CIA in Guatemala: The Foreign Policy of Intervention* [1982]) or Gregory F. Treverton (*Covert Action: The Limits of Intervention in the Postwar World* [1987]), but they ought at least to address the arguments if only to show students how mistaken these other scholars are.

The book is not dominated by this kind of treatment, however. It shows up here and there like a burst of bad temper, easy to spot and easily compensated for. One blessing of Duignan and Gann's conviction of right is that there is never any doubt as to where they stand on any issue, large or small.

One final point: the "Americanization" of the subtitle is not to be taken too seriously; the authors use it mainly as a passing reference, not as an organizing concept that is somehow distinct from modernization. These caveats aside, we still have a big book that treats a big subject in a grand and magisterial fashion. It is comprehensive. It is convenient to use. It is an impressive accomplishment.

DOROTHY V. JONES
Newberry Library

ODD ARNE WESTAD. *Cold War and Revolution: Soviet-American Rivalry and the Origins of the Chinese Civil War*. New York: Columbia University Press. 1993. Pp. x, 260. Cloth \$50.00, paper \$16.50.

In this impressive reappraisal of the origins of the Chinese civil war, Odd Arne Westad touches on an important aspect of Cold War history: the interactions between the superpower competition and Third World revolution. Covering the period from mid-1944 through May 1946, he carefully studies the complexities of such important events as the Yalta Conference, the Sino-Soviet talks in Moscow, the Soviet and American armed interventions, the Chinese Communist Party–Guomindang rivalry in north and northeast China, and George Marshall's mediation mission. Indeed, this is the best study of these events since Steven I. Levine's *Anvil of Victory: The Communist Revolution in Manchuria, 1945–1948* (1987).

Like Levine, Westad employs an international and multiarchival approach. His study follows "a four-cornered pattern of analysis" (p. 3) to inquire into the policies and actions of each player: the United States, the Soviet Union, the Guomindang, and the Communist Party. It is also important to note that Westad is one of the few who has extensively used the recently available Chinese-language archival materials in mainland China and Taiwan. Not long ago historians went to Taiwan to obtain Communist Party documents and to the mainland for Guomindang materials. Having gained access to such archives as *Zhongguo Dier Lishi Dang'anguan* (The Number Two Historical Archives of China) in Nanjing and *Guofangbu Shizheng Bianyi Ju* (The Ministry of Defense's Bureau of Compilation and Translation) in Taipei, and having used such collections of documents as *Zhonghua Minguo Zhongyao Shiliao Chubian* (Preliminary Compilation of Important Historical Materials on the Republic of China, Taipei, 1980–85) and *Zhonggong Zhongyang Wenjian Xuanji* (A Selection of CCP Central Committee Documents, Beijing, 1985–87), Westad is in a better position than many others to cross-examine Communist Party and Guomindang foreign policy of the 1940s. Although unable to use the newly declassified Soviet documents—Stalin's papers and the central committee of the Communist Party of the Soviet Union archives were not available when the book went to print—he vividly sketches the Kremlin policy toward China in the light of both Communist Party and Guomindang archival evidence.

As a fairly traditional study of diplomatic history, this book concentrates on the perceptions and actions of American, Soviet, and Chinese governments, and the Chinese Communist Party leadership. Shaped by the state-oriented approach to international history, Westad assigns priorities of his analysis to Stalin, Franklin Roosevelt and Harry Truman, Jiang Jieshi (Chiang Kai-shek), and Mao Zedong, whose roles are most extensive in official documents and private papers in the United States, Taiwan, and mainland China. By examining the Communist-Guomindang power struggle in the Cold War context, he makes an intriguing argument: the Soviet-American rivalry "facilitated a rebel victory by making it impossible for the government to mobilize foreign support . . . [and thus] opened the door for third world rebels to exploit the international great power system for their own purposes" (p. 178). Westad's major contribution to the historiography of the Cold War in Asia results from his convincing argument that, in the immediate post-World War II months, both the Guomindang and the Chinese Communist Party made consistent efforts to manipulate the great powers into expanding their political interests.

Nevertheless, Westad's purely realist stance is troublesome. One problem is the "moral equivalence" doctrine hidden in the book: he evidently treats each decision maker as equally rational. Here we are talking about Stalin, Roosevelt, Truman, Jiang, and

Mao; it would be surprising if they all shared the same cost/benefit calculus in their foreign policy consideration. If nothing else, possible clashes of different personalities—or, perhaps, different beliefs, aspirations, attitudes, values, or mind-sets—might have rendered “realistic” foreign policy extremely difficult and might well have been sources of misperception or miscalculation of each other’s foreign policy.

There can be little doubt, however, that this is an excellent study on the origins of the Chinese civil war and an important addition to the historiography of the Cold War in Asia.

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JANE HAMILTON-MERRITT. *Tragic Mountains: The Hmong, the Americans, and the Secret Wars for Laos, 1942–1992*. Bloomington: Indiana University Press. 1993. Pp. xxviii, 580. \$29.95.

Although several aspects of the Southeast Asian war that America walked away from between 1973 and 1975 have been well documented, the war in Laos has remained shrouded in the mists of time. This is partly a result of the difficulty of talking to the players and partly due to the reluctance of the U.S. government, even at this late date, to acknowledge that anything was happening in that Land of a Million Elephants. Students of Southeast Asia and former participants of course are well aware that fierce fighting raged throughout Laos during this period, but the detailed story of what happened has eluded us.

Jane Hamilton-Merritt, a former reporter who covered the Vietnam War for both midwestern and Asian newspapers and magazines, lived at the edge of the story of the secret war in Laos. Sympathetic to the Hmong and other Lao tribal groups, Hamilton-Merritt sets out to give them a voice in this book, which is distilled primarily from her interviews with Hmong and the Americans who worked with them. She follows the Hmong from the 1940s, when the outbreak of rebellion against French colonialism first made Laos a battleground, to the 1980s, when a communist successor regime continued fighting the Hmong, by now former American allies in the Laotian secret war carried out by the Central Intelligence Agency (CIA).

Evoking the pain and grief of the Hmong, Hamilton-Merritt’s book suffers from the author’s abandonment of scholarly detachment to assume an activist role. Hamilton-Merritt had a leading part in advancing claims of communist chemical warfare in Laos, and she uses a great deal of this study to attempt to beat back the criticisms that have brought that charge into serious question. Hmong human rights are important, and Hamilton-Merritt can be commended for honoring them, but it is not necessary to believe

in chemical warfare to think the Hmong should be helped.

Animosities between lowlander Lao and the mountain tribes are endemic and traditional, and the Hmong alliance with the CIA only made it even more likely that repression would follow. Yet this feature is almost absent from this book, which takes the United States and Hmong virtually in isolation to emphasize the theme of American betrayal. Nevertheless, social mobilization of the Hmong necessary to create a CIA army inevitably carried seeds of further conflict with the Royal Laotian Government (RLG). It is not sufficient for the author to accept third parties’ assertions that Vang Pao desired only assimilation into Lao society. Not only is this in conflict with documentary records now becoming available but also the basic U.S. alliance was with the RLG, which left the CIA with an inescapable contradiction in mobilizing the Hmong. The initial Hmong decision to cast their lot with the CIA merits deeper study from both sides than it receives here.

It is also paradoxical in this account of U.S. betrayal of the Hmong that the onus is not on wartime leaders for the act: rationales given by the Vientiane embassy for all kinds of Laotian matters are repeated here as gospel, giving the treatment a general lack of depth. The view that Hmong were unique among combatants is belied by the parallel CIA/military mobilization of Montagnards in South Vietnam, who if anything have been treated even more shabbily since the U.S. war (there are 220,000 Hmong in the United States today, as against fewer than 1,000 Montagnards). The assertion that the CIA never lost a secret war until Laos (p. 366) would surprise readers who are Cuban, Indonesian, Kurdish, or Tibetan. Read this book for its Hmong voice, not its history.

JOHN PRADOS
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ANCIENT

CHRISTOPHER A. FARAONE. *Talismans and Trojan Horses: Guardian Statues in Ancient Greek Myth and Ritual*. New York: Oxford University Press. 1992. Pp. xii, 193. \$35.00.

A closely argued analysis of the methods and means of averting evil and bringing good fortune in ancient Greece, Christopher A. Faraone’s volume bears the earmarks of being a dissertation, slightly modified for publication. Expansive explanation is sacrificed to the amassing of detail (often, to be sure, through the welcome citation of ancient texts about individual points), the result being more accessible to the specialist scholar than to the educated general reader. Elaborate and numerous footnotes follow each chapter, occasionally otiose but more often pertinent, helpful supplements to the thesis. The book is arranged in seven chapters and four appendixes, plus a bibliography that deserves praise for its comprehen-

siveness (even if it is not always up to date); in it, students of Greek magic and religion are provided a rich array of secondary works.

The introductory chapter sets forth an important distinction between talismans and apotropaic images. The former was normally secreted within the walls of a city, protected by its presence alone, whereas the latter was set up at a gate or boundary to frighten potential invaders or wrongdoers. Faraone plausibly argues, here and throughout, that apotropaisms are "functional" rather than "symbolic" (pp. 9–10); they defend through innate power rather than just serving as a reminder of the community's right and ability to defend itself.

The theory that talismans defend from within is perhaps best illustrated in the close study in chapter 6 of the Trojan horse story, as it appears in the *Aeneid* and elsewhere. In his commentary on Vergil, Servius (late fourth century) offers six explanations for this celebrated tale about the fall of Troy. The first of these, that the horse is a poetic metaphor for a battering ram, was for centuries accepted by ancient and modern critics. In recent decades, however, the episode has been more commonly seen as a literary version of a myth of a horse-Poseidon or simply as a fertility symbol. Faraone prefers, however, the last of Servius's possibilities, that the horse was as Vergil described it: a wooden statue, sacred to Athena, to replace the stolen Palladium that had given internal protection to Troy, which explains the Trojans' eager acceptance of it. Faraone adduces further evidence, involving the Pandora legend and Medea's trick in entering Iolcus, to postulate the existence of a hitherto unrecognized genre, one involving a "ruse of the talismanic statue" (pp. 100–02).

Apotropaisms are given a penetrating discussion in the final chapter. Guardian images such as gorgons are well known, but in addition we should consider eastern Greek dog and lion statues at city entrances and bow-bearing plague gods, probably Anatolian in origin, set up before gates. Other chapters lay out evidence from Greece, Egypt, and the Near East concerning such diverse images as severed heads, theriomorphic demons, amulets, and effigies, along with binding-spells and statues, ghost-banning rituals, and other phenomena of aversion and protection, public and private.

The author argues for a "persuasive" and "manipulative" role for Greek religious art, rather than one of description or symbolism (pp. 120–21). He does so convincingly, consistently bolstering his assertions with tangible and literary evidence. Because he examines various kinds of evidence from Babylonia, Mesopotamia, and elsewhere in Asia Minor, it is a pity that there is no mention of the Babylonian *kudurru*, boundary stones bearing lengthy imprecations that may have influenced early Greek epigraphical conventions. But this is one disappointment among many revelations in a work of persuasive complexity. Evidence from eclectic sources is assembled to serve as

the foundation of significant new theories for the study of Greek religion, magic, ritual, and the art associated with them.

ROBERT L. POUNDER
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CARLIN A. BARTON. *The Sorrows of the Ancient Romans: The Gladiator and the Monster*. Princeton: Princeton University Press. 1993. Pp. 210. \$24.95.

William L. Langer suggested in his AHA presidential address of 1957 that psychoanalysis might be a beneficial tool to historical biographers and that historians generally might examine the "collective mentality" even on the unconscious level, because "communities, like individuals, can be profoundly affected by some shattering experience" ("The Next Assignment," *AHR* 63 [1958], 290–91). Carlin A. Barton believes the collective mentality of the late Roman republic and early empire was profoundly affected by chaotic civil wars and the rise of absolutism that accompanied the principate. This richly textured book "maps" Roman emotions—despair, desire, fascination, and envy—in an attempt to understand how that society and individuals in it, "at and beyond the limits of endurance" (p. 3), coped with the imbalance created by despotism. Gladiators and monsters are the "figures through which . . . extremes of emotions were enacted and expressed" (p. 4). They are "mirrors of the absolute, inhabiting the territory at, and outside, the limits . . . They are animated metaphors for the most disturbed aspects of Roman life, allegories of the extreme" (p. 187).

Barton argues that ancient Romans thought in homeopathic terms—that "like things are cured by like" (p. 180)—and that they strove to maintain an equilibrium by means of contests, both serious and playful, between reciprocal opposites. Extraordinary skill was required to keep the bipolar forces in balance. Barton would doubtless agree that it was not the civil war that destroyed the republican aristocracy, but the murderous peace of the principate that tipped the scale to one extreme.

Everyone knew that there was something wrong with the principate; Barton argues that it was the loss of the ability to act honorably. Honor was won, as in the case of the gladiator, in a contest between equals. The gladiator was a metaphor for empowerment because, when he willingly fought to the death, his heroic act ennobled himself, those who controlled the games, and the audience. Under tyrants, on the contrary, status and survival at court depended on humiliation, compromise, and sycophancy, resulting in the loss of honor. Consequently, because gladiators, like martyrs, did not fear death and willingly sacrificed themselves as a noble and welcome fulfillment of their obligation, this resulted in an inversion of past standards of honor and nobility. Inversion is the major theme of the book because in a time of

crisis—like civil war or despotism—things are not what they seem. “Each extreme turns back on itself” (p. 157) and becomes its opposite, so that gladiatorial games and monsters created by excessive control actually mitigate it and become “the chief heroes in the attempt to reestablish a fluctuating, compensatory system” (p. 189).

Barton amasses an impressive collection of ancient evidence and treats it to an even more impressive interpretation, reinforced by references to modern psychological and anthropological studies. The thesis is enriched and underscored by countless examples from contemporary films, plays, and literature, which is not to say ancient scholarship is neglected. This provocative volume deserves a wide audience, but it remains to be seen whether classical scholars generally will make use of it given their standard isolation from other forms of scholarship and their faith in the homeopathic nature of classical material, their conviction that only classical evidence can be used to elucidate the classical world.

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MANFRED FUHRMANN. *Cicero and the Roman Republic*. Translated by W. E. YUILL. Cambridge, Mass.: Blackwell. 1992. Pp. viii, 249. \$34.95.

We know more about Cicero than about any other person from antiquity. Besides Plutarch's *Life of Cicero* and references in contemporary and later histories, nearly a thousand letters to and from Cicero's friends and dozens of his orations and essays survive. But even a period of antiquity so important as the first century B.C. is poorly documented, and reconstructions of most significant events are still uncertain. As a result, the rich surviving material gives us a firmer picture of Cicero's inner life than of the events in which he participated. An effective biography, then, must get a good hold on his complex psyche while giving a plausible depiction of the intellectual and political world he helped to shape.

Manfred Fuhrmann's book, unfortunately, is not up to the challenge. Fuhrmann states: “I am concerned above all to present Cicero's life to those readers who do not possess extensive previous knowledge of the subject and to whom ancient languages and the history and culture of the ancient world are relatively unfamiliar” (pp. vii–viii). Fair enough, but the lay reader deserves the same careful scholarship demanded by the professional. The First Triumvirate, for example, should no longer be characterized as “quasi-revolutionary” (p. 72). The explanations of *nobiles*, *optimates*, *populares*, and *equites* are old-fashioned and static, depriving the reader of an appreciation of the rich complexities of Roman politics. Fuhrmann, an authority on ancient rhetoric, is better at summarizing Cicero's rhetorical and philosophical

writings, but his lack of political understanding undermines the result. His discussion of *De officiis* (*On Duty*) fails to highlight the political naiveté of Cicero and his senatorial colleagues (pp. 183–87). Written just months after Caesar's assassination, the work expressed the futile hope for oligarchic restoration. Cicero also there attempts to demonstrate that Caesar had always been bent on destroying the senatorial order, and Fuhrmann falls under the sway of this distortion when recounting Caesar's role in the Catilinarian conspiracy two decades earlier (pp. 62–70).

Although this is a work designed for a lay audience, it presumes much knowledge. Fuhrmann notes that Marius exerted a strong influence over Cicero (pp. 15–18) but never explains the reasons for Marius's great fame (alluding only to Marius's disastrous actions in the 80s). Some quotations from ancient authors and titles of works are translated, but others are unaccountably left in Latin (with Greek titles, the awkwardness is compounded: “[Cicero's] *De Republica* . . . like Plato's *Politeia* and St Augustine's *City of God*” [p. 113]). Originally composed in German, the work contains a bibliography consisting almost exclusively of German titles (many out of date). There is no reference to or apparent consultation of D. R. Shackleton Bailey's text and magisterial commentary on Cicero's letters (*Cicero's Letters to Atticus* [1965]; *Epistulae ad familiares* [1977]; *Epistulae ad Quintum fratrem et Brutum* [1980]), nor consideration of most of the recent work in the field. Fuhrmann succeeds in adequately describing Cicero's literary output. But there are several other recent biographies of Cicero to which the reader, ignorant or deeply knowledgeable of Roman history, can turn for more accurate pictures of Cicero within the Roman Republic.

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PETER BROWN. *Power and Persuasion in Late Antiquity: Towards a Christian Empire*. (The Curti Lectures, 1988.) Madison: University of Wisconsin Press. 1992. Pp. x, 182. Cloth \$45.00, paper \$12.95.

Peter Brown's latest offering is marked by the elegance, wit, and imagination that Brown's readers have relished in the past. This wide-ranging volume describes the shift that altered power relationships in eastern Mediterranean cities of the late fourth and early fifth centuries. The main actor in this drama was the local Christian bishop, who displaced the civic aristocracy as the principal patron and “nourisher” (p. 82) of the urban populace, and who on a wider stage usurped the traditional role of the pagan philosopher (frequently a member of the urban elite) as the city's mediator with the emperor. In this last role, the bishop enjoyed the right of philosophical *parrhêsia*, the ability to sway and forthrightly advise emperors based on the speaker's own moral authority.

Readers of Brown will recognize the familiar outlines of a scenario played out in the countryside, where the Christian holy man, endowed with supernatural authority, usurped the landed *potentiores* as patrons of rural society. In an urban setting or in the imperial court this transformation was modulated by a host of different variables, and Brown skillfully employs comparative data from the absolutisms of early modern Europe in order to explore the complexities of the process.

Before he charts this shift in urban power relations, Brown describes with particular verve the realities of government in late Roman cities. He evokes for us the difficulties faced by a provincial governor as well as the fears and pretensions of a civic aristocracy bred on classical *paideia*. The ever-increasing brutality of imperial government is strikingly portrayed, as Brown describes the "tide of horror [which] lapped close to the feet of all educated persons" (p. 52). Brown's attention to detail and to the inner life of his protagonists results in a far more vivid treatment of late imperial administration than the usual sterile fare redolent of the *Codex Theodosianus*.

The crux of Brown's argument, a chapter entitled "Poverty and Power," traces the successful maneuvers of the local bishop in his bid to wrest control of urban power structures from the hands of the traditional elite. Brown jettisons the model of a simple conflict of religions during the mid to late fourth century. This shopworn model simply parrots the triumphalist views of Christian chroniclers. Instead, Brown envisions the strife endemic to late Roman cities as "a struggle for a new style of urban leadership" (p. 77), in which the bishops broke the aristocratic monopoly on civic euergetism, carved out new sources of support among the poor, and eventually gained control of the resources of the city. This is a dense chapter, sure to elicit lively reaction from those working on individual bishops, controversies, and cities. If we are forced to demur from his interpretation of particular incidents, this does not detract from the persuasive power of his overall synthesis.

The rise of the Christian bishop was accompanied by the articulation of a new language of power developed by Christian writers. They created "a mirage of a totally Christian empire" (p. 131) in which paganism was utterly vanquished and its exponents were chased into the shadows. Brown argues instead that numerous pagans continued to thrive, aided by "an ideology of silence" (p. 128) on the part of Christians who refused to admit their existence. How do we gauge the pervasiveness of polytheism under these conditions? One body of evidence, not fully exploited by Brown, consists of the material remains offered up by excavations in late Roman cities. Beyond the dazzling statues of philosophers and the inscriptions carved on the seats of theaters and hippodromes, more prosaic items of everyday use might betray the religious predilections of the humbler classes, the groups that the bishops claimed to repre-

sent. Despite these minor reservations, Brown's book is sure to take a deservedly central place in the ongoing debate over the dynamics of late Roman society.

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MEDIEVAL

HUGUETTE TAVIANI-CAROZZI. *La principauté lombarde de Salerne (IX^e-XI^e siècle): Pouvoir et société en Italie lombarde méridionale*. In two volumes. (Collection de l'École Française de Rome, number 152.) Rome: École Française de Rome. 1991. Pp. lxxvii, 676; 680-1203.

The Lombard principalities of southern Italy are still a neglected topic. Sandwiched between the southern edge of the Carolingian and post-Carolingian world and the western edge of the middle Byzantine empire, prone to fission and local war, and eventually swept away by the more glamorous Normans, they have seemed a marginal historical curiosity. But one could put it another way: along with the Anglo-Saxon and Celtic world, and the kingdoms of Asturias and León, they are the alternatives to Carolingian hegemony, instances of how the world developed outside what Giovanni Tabacco has called the "ambiguity" of Frankish institutions. And of all these alternatives, Salerno is for its size by far the best documented in the two centuries or so of its existence, 849-1077, thanks to the Cava archive. Huguetta Taviani-Carozzi is well aware of Salerno's special position as offering the best place for "the analysis of an original conception of power" (p. xiv), and dedicates to it a long book, in the best traditions of large French regional *thèses*.

Taviani-Carozzi's task is, in fact, harder than most such *thèses* for this period in one crucial respect: although Salerno was a small state—about the size of Anjou—it had a complex governmental tradition, inherited without a break from the Romano-Germanic kingdoms, which cannot be ignored. She indeed spends a large percentage of her book (pp. 241-676) discussing it. She takes us through the rules of princely succession, central and local government (official by official), law and judicial institutions, and the organization of the church. She then, in the last third of the book, sets this against a characterization of political society in the principality and its eleventh-century breakdown (of which the Normans were only a powerful element, not the sole cause). Here she stresses the rules of social bonding between the prince and his *fideles*, and the separation between the tenth-century aristocracy and local power (which was long controlled by public officials such as judges); and then the eleventh-century entry by these judges into the ranks of the aristocracy and the subsequent drift to local power controlled by judicial aristocrats and princely cadets and, eventually, churchmen. The Normans simply inserted themselves into this move

toward decentralized power and exploited it for their own ends.

Taviani-Carozzi has created a rich characterization of Salerno's political society. I would single out, as some sections that I found particularly valuable, the discussion of the political role of the major princely private church, S. Massimo di Salerno (pp. 409–39); the analyses of law, where she shows how little difference there really was between Lombard and Roman law (pp. 515–25); the discussion of gifts, which she shows to be not only much less hierarchical than those in the Frankish world but also conveying much less permanent obligations (pp. 685–725); the neat analyses of the ascent of judicial families (pp. 772–800); and the characterizations of the developing seigneuries of the mid-eleventh century (pp. 865–97). Overall, although she is certainly capable of getting lost in detail, she manages to balance her institutional discussions with her propositions about the development of social and political bonds inside the aristocratic world, to produce a carefully structured whole. She proposes, in fact, that Salernitan society was remarkably stable for a long time, throughout the tenth and early eleventh centuries, with few tendencies to decentralization, largely because the aristocracy was kept close to the prince's court and away from local political responsibilities; the lands of each family were scattered across the principality, and the army (and military activity in general) was small, thus weakening two of the main motors of movement away from central power. The localization of the mid-eleventh century was thus a new process, and happened for reasons separate from those of the Frankish world. These arguments are broadly convincing and make the book necessary reading for any future student of tenth and eleventh-century politics in Western Europe.

I only have two criticisms of the book. The first is that Taviani-Carozzi is something of a Lombard legitimist: Salerno is the lineal heir of the Lombard kingdom, and therefore her major point of reference is eighth-century Italy rather than the societies contemporary with the Salernitan principality. I can see why she wants to avoid easy Frankish comparisons; she does not want northern France to be the "normal" development and southern Italy merely to be an exception that needs explaining. But her choices lead to an excessively timeless quality throughout the book, until she gets to her eleventh-century changes. They also lead her to the considerable misjudgment of including a long chapter (pp. 97–169) on Paul the Deacon's and Erchempert's Lombard histories, which are not only irrelevant to her main themes but also contain an extremely unconvincing attempt to fit these historians' views of sovereignty into the strait jacket of the theories of Georges Dumézil. (She would have benefited from, among other things, knowing something about the Anglo-American tradition of folklore studies.) This is a bad start to the book, which the reader takes time to recover from.

The second problem is that she is not very interested in one major French historiographical issue, the nature of local political power. Even her seigneuries are seen from the top down, not the bottom up. There are as a result a series of problems about the local construction of power that are skated over. Why, for example, were aristocrats content to stay in the prince's orbit for so long? Taviani-Carozzi shows how rapacious and cynical they could be in the ninth and later eleventh centuries; what happened in between? She insists on the rule of law, but also on the weakness of the army; how, then, was that law enforced? She argues, rather briefly (pp. 512–13, 863–64), that her castles are unlike those in Pierre Toubert's Lazio; what were they for, then? There are almost no peasants in this book, or even local notables; the affairs of the aristocracy are as a result insufficiently grounded in the agrarian society from which they got so much of their wealth. This is a pity, for there is so much else, and this would have tied it up. We are left with as many questions as answers. But we have learned a huge amount in the meantime.

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MICHAEL CAMILLE. *Image on the Edge: The Margins of Medieval Art*. (Essays in Art and Culture.) Cambridge: Harvard University Press. 1992. Pp. 176. \$35.00.

Copulating figures and scatological images in the borders of manuscripts; freakish humans, chimeras, and prostitutes among the sculptural embellishments of churches and monasteries: this barely begins to suggest the extraordinary variety of "marginal" subjects in the margins of late-medieval art. The arousing power of these works has engendered fascination, but first prompted (as their creators might have wished) discomfort, confusion, and sometimes even vilification. St. Bernard's "deformed beauties and beautiful deformities"—his often-cited characterization of one grouping of such works from his "Apologia" to William, Abbot St.-Thierry of about 1150—have now received their due. The elusive functions and meanings of these figures are examined, or, better, celebrated by Michael Camille in a book that is at once a delight to read—a provocative and challenging read—as well as a scholarly milestone in the exploration of the "anti-world" of the Gothic imagination.

Camille's scintillating writing and intelligence quickly draw us into this fabulous realm and invite us to share the author's passion for this remarkable material. The postmodern focus on the marginal, as Camille himself points out, renders this a fitting subject for our times, although it is not the least among the virtues of the book that Camille does not burden the reader with comments on methodological strategies even as he invokes a wide range of author-

ities (Sigmund Freud, Mary Douglas, Edmund Leach, and Norman Bryson, to cite but a few).

Treating Northern European works of the 1100s to mid-1300s, Camille looks first at the borders of sacred manuscripts, then at the capitals of monastic cloisters and at the decorations in the barely visible, shadowy regions of the interiors and exteriors of churches, moves next to the margins of courtly manuscripts, and concludes with a study of the boundary art (understood here, as throughout, in both a literal and figurative sense) of civic representation. In the course of the journey through these spheres Camille draws on his broad command of medieval life and culture to suggest brilliant new interpretations of many of the themes of this netherworld—of, for example, the figure pinned with his nose to the choir stall seat, the woman offered the feces of her loved one, or the figure bending over to expose his buttocks to the lance of an equestrian monkey. Camille shows how the image on the edge amplifies, comments on, or satirizes the more conventional and orthodox sacred and courtly meanings at the center of the manuscript, monastery, and church.

Scatological representation comes to stand for a chivalric order in crisis and decline ("Courtly Crap," Camille titles this section of the study). Deformed humans function, in part, as visual manifestations of the close interaction between man and beast and of the anxieties that come of this association. Gargoyles, to note one further conclusion reached by Camille, are emblems and embodiments of fears joined to comic and parodic intentions; a discussion that leads to a superb passage on the nature of the borders of the sacred space of ecclesiastical structures, an unexpected turn typical of the delights of this book. A medieval world view of polarities emerges: at once remarkably open to the free expression of artistic creativity—indeed, a society that was in some measure dependent on such expression for its survival—yet a society equally marked by strong prejudices (against women, for example). This is the medieval mind as wrought by Camille.

The staggering show of the power of imagination chronicled by Camille is matched by his own imaginative reconstructions. This is truly a spectacular book, one that is only slightly marred by the absence of an index and by its brevity. One wants to read more by Camille on this subject.

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MODERN EUROPE

ANTHONY PAGDEN. *European Encounters with the New World: From Renaissance to Romanticism*. New Haven: Yale University Press. 1993. Pp. vi, 216. \$25.00.

Anthony Pagden seeks to explore the "implications for Europeans of their encounter . . . with the New World" (p. 1), and he succeeds best in lucidly repre-

senting eighteenth-century French and English writers. He also presents an interesting analysis of the writing of the French philosophes on the New World and usefully summarizes such frequently forgotten eighteenth-century writers as William Robertson. But the problem here is that this account lacks comparative perspective, and without this Pagden can only retell (however elegantly) the tales that have already been told, tales that have reached the point of mythos, about the European discovery of the Americas.

Pagden, for example, rightfully attributes significant change to critiques, widely disseminated during the sixteenth century, of a traditional medieval belief that the tropics were uninhabitable. But he seems unaware that this tale was in fact developed much earlier by Portuguese navigators and traders encountering large civilizations in equatorial Africa. Their criticisms of traditional textual authorities were based on the twin criteria of experience and eyewitnessing. Furthermore, their criticisms were reconfirmed through direct contact early in the sixteenth century with Indonesia, which was as densely settled as equatorial America. Thus, the issue of the Americas must be placed within the broader historical context of exploration to get beyond the "first contact" syndrome that characterizes contemporary historical writing, especially that focusing on the discovery of the New World. Earlier works composed by Portuguese navigators were channeled to Europe through their German and Italian collaborators. The question that needs to be asked is how, if at all, did the discovery of the Americas change or reshape perceptions of this earlier, and indeed more global, phenomenon?

Missing in Pagden's treatment of how contact with the Americas affected Europeans are considerations of the readerships: to whom are these constructions of America being addressed, and by whom are they being read? There is no focused treatment, for example, of reception, or of which Europeans were participating in debates about the construction of America. Indeed, the New World was being constructed for Europe in contexts far broader than those considered by Pagden. It was constructed in letters written home by priests and ordinary citizens during the sixteenth century, in ships' logs and diaries, and in theater and fiction. If America was being constructed by multiple European voices, and in multiple European contexts, then focusing on a few well-known published sources seems narrowing and sometimes misleading, particularly when the connections of those texts both to other texts and to other experiences remains unasked and unanswered.

Finally, there is indeed considerable value and meaning to be found in examining the European dimensions and implications of the colonial experience. Pagden, however, ends curiously with a statement of universal moralism: "We all, it seems, need to salvage some notion of ourselves as potentially benevolent agents to persuade ourselves that European

civilization is not quite so rapacious and destructive" (p. 187). "¿We all?"

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NICHOLAS V. RIASANOVSKY. *The Emergence of Romanticism*. New York: Oxford University Press. 1992. Pp. viii, 117. \$29.95.

To me, few topics are as interesting as the emergence of romanticism. The question of how and why any major turn in thinking and perceiving happens is of course fascinating and elusive. The concerns of the Romantic movement, moreover, are still very much with us. It is difficult even to talk about our culture today without trying to understand its Romantic roots; such is the strength and continuity within the core of our culture. The question of the appearance of modern intellectuals has never, to my knowledge, been satisfactorily addressed in its relation to romanticism, and the establishment of *la classe pensante* accompanies the ultimate decapitation of aristocracy. Nicholas V. Riasanovsky addresses these and other related questions in his interesting book, a slim volume that is a stimulating and satisfying contribution to European intellectual history.

Riasanovsky chooses for analysis the end of the eighteenth century, that is, a moment when romanticism appears. The book is divided into three studies. The first is devoted to the emergence of romanticism in England. It discusses the early poetry of William Wordsworth, the centrality of his philosophical vision of nature, and his unusual collaboration with Samuel Taylor Coleridge. One of the topics Riasanovsky addresses in this essay is the complexity of artistic and intellectual authorship. Even in the case of such outstanding Romantic individuals as Wordsworth and Coleridge, their work resulted from collaboration rather than from single wills. Moreover, there are generational roots to a new outlook or new world view, and even the most reclusive authors built their work in the spirit of the moment, however minoritarian. Wordsworth's original and individual pantheistic vision of nature was shared (and then abandoned at about the same time) by several early English and German Romantics.

Indeed, the second essay points to parallels between early romanticism in England and Germany. The poets Riasanovsky analyzes—Novalis, Wilhelm Heinrich Wackenroder, Ludwig Tieck, and Friedrich Schlegel—show a similarity of vision and of timing that cannot be accidental. "In Germany, as in England," Riasanovsky writes, "romanticism emerged strikingly—indeed as an explosion—to be followed by collapse and retreat and to be succeeded by new romanticists, related to the original figures but also distinct and different" (p. 67). It is striking that the similarities of concerns and interests, and the affinities in outlook are common to the most personal,

most individual expressions of each Romantic solitary poet.

In the third part of the book, an essay modestly entitled "Some Observations on the Emergence of Romanticism," Riasanovsky sketches some influences that may be at the root of these similarities. The main context within which Riasanovsky places the appearance of the Romantic outlook in German, British, and, later, Russian cultures is Christianity. (One unusual characteristic of the book is the range of cultures addressed. The pages devoted to Russian romanticism prove once again that the divide between Eastern and Western European cultures serves political rather than intellectual purposes.) Riasanovsky treats Christianity not only as a religious belief but also as a common, in this case European, cultural background within which romanticism is explicable. I certainly agree with such an approach. Its limitation, however, is that it explains those elements within romanticism that preserved the continuity of European culture better than it does the new ones. Yet romanticism brought above all a radical change in perception and world view. It is the new that needs to be explained.

Riasanovsky discounts (in my opinion too radically) political (that is, historical) events as the major reason for the appearance of some Romantic traits: the unrelenting despair, the search, never to be fulfilled, for unity between the unhappy poet and nature-universe-God. He points to many reasons for the change, holding that a cultural transformation of such magnitude cannot have a single cause. It is difficult not to agree with such an undogmatic and cautious approach, particularly inasmuch as it produces interesting ideas and suggestions. Yet the temptation to ask, while discussing the emergence of a major turn in European thought, what possibly may have caused it is irresistible. A way of satisfactorily succumbing to such a temptation, in my opinion, is to abandon the rigid separation between political events and cultural or philosophical changes. Romanticism was a way of conceptualizing and poeticizing (or expressing) a new relationship between the individual and God. That basic relationship influenced everything, including the understanding of what social stability and the individual's place in society were. Riasanovsky writes that romanticism internalized into its world view the Christian sense of the imperfection of the individual, and of the individual's desperate search for unity with God. That sense of isolation and despair, as well as the retreat of major early Romantics into right-wing ideologies after 1806, may well have been related to the growth of equality and the undoing of social structures that led to and followed events such as the French Revolution. Romanticism was, after all, a way of talking about the individual as a separate and abandoned being.

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IDA ALTMAN and JAMES HORN, editors. *"To Make America": European Emigration in the Early Modern Period*. Berkeley and Los Angeles: University of California Press. 1991. Pp. vii, 251. \$34.95.

IDA ALTMAN. *Emigrants and Society: Extremadura and America in the Sixteenth Century*. Berkeley and Los Angeles: University of California Press. 1989. Pp. viii, 372. \$45.00.

If anything is left of the image of a single young man crossing the Atlantic to a shining future, an image whose analogue was the figure of Christopher Columbus, these volumes may give it the *coup de grâce*. As these two books tell it, whereas most such young emigrants did so aspire and the common denominator spurring emigration was perceived opportunity, those who made their way most successfully, economically and psychically, were almost always bolstered by connections in America and by family ties maintained in Europe. A bundle of factors, not a single vision, usually lay behind decisions to emigrate, and such decisions were most often made by heads of families. These studies not only tend to further undercut the creed of triumphant individualism that was trashed in 1992 but they also advance some concepts then presented to supersede it, especially that of all America as an entity, one made up of culturally diverse communities having the family as a basic unit. They also agree that most Europeans emigrating to the New World from the sixteenth through the eighteenth centuries, whether English, Spanish, French, or German, came from the working poor and remained among them, that most arrived in some sort of servitude, for longer or shorter terms, that it was in the later periods of more massive, more impersonal immigration that such people endured the most abusive treatment, and that it was the young, solitary migrant who suffered most.

Ida Altman and James Horn, editors of *"To Make America,"* state that "historians must abandon traditional national perspectives that have artificially set Europe apart from America and the experiences of one colonizing nation from those of others" (p. 22). Yet if previous work has been along national lines, the studies in this collection are not exceptions. Rather, its newness lies in bringing together pieces on immigration to European colonies throughout America. It is at bottom an offering to the social history of New World diversity and a testimony to the majority of Europeans who crossed the Atlantic neither slave nor free, in an emigration, as some of the contributors note, that was but the farther extension of migration within European imperial entities. The exceptions are telling although unexplored as such: the Germans who went to British colonies, Spaniards coming with French from La Rochelle, and people of African origin or ancestry from Seville. The contributions test-bore into a variety of times and places. The questions raised also vary, as does the quantity and quality of previous research available to each author

and the historiography behind each approach. Both books exhibit faith in history as cumulative, in contributing to a pot of data for future scholars to interpret.

Horn draws to advantage on a large body of previous work in looking at patterns of British emigration to the Chesapeake region in the seventeenth century. Altman builds on her book on emigrants from Extremadura to Spanish America, while Auke Pieter Jacobs provides information on people sailing from Seville, 1550–1650, legally and otherwise. Leslie Choquette addresses recruitment in France for Canada, 1600–1760. Christian Huetz de Lemps looks at indentured servants going to the French Antilles in the 1600s and 1700s. Finally, in an analysis both broad and precise, Marianne Wokeck interprets the dynamics of German immigration to British North America, 1683–1783.

Emigrants and Society, by Altman, reflects admirable familiarity with difficult archival material and presents much new detail concerning emigrants and their networks of kinship and friendship within two towns in one Spanish region, Extremadura. The author seems to strive to revise the traditional view, of Spaniards as set apart from other Europeans in coming to America for glory, God, and gold. Pressing the point that most of the people who left Extremadura (usually for Peru in what they referred to as the Indies) were seeking opportunity, Altman does bring Spanish movement over the Atlantic closer to the European norm. Still, neither this book nor her essay in *"To Make America"* state plainly the old truths they in fact corroborate, that Spaniards alone found precious metals and sedentary peoples in America and sought to reap the advantages of both. Understated in both books, it remains a very important difference that there was no need in Spanish America for numerous heavy-duty laborers from home, and that, conversely, other Europeans neither initially found gold or silver nor had the possibility of dominating a New World labor force.

The detailed evidence Altman offers in *Emigrants and Society* for the Extremaduran towns of Cáceres and Trujillo is at times at variance with the social profile she draws. Thus, the nobility is discussed as an idle affluent class although—unstated, well-known, and indicated by evidence scattered throughout—its members, relative newcomers to their titles, worked hard at war and in business ventures. Convinced that he who stands still falls behind, they dominated towns and other, less-powerful nobles and served as models within their societies for upward mobility. Patently, although played down in the telling, society was hierarchic, and *extremeños* perceived that the most certain and prestigious way to individual wealth and power lay through military endeavor that could yield booty, royal grants, and higher status. In the sixteenth century, Extremadura, a Spanish backwater long notorious for violence, remained a society geared for war, one wherein the line was nearly

nonexistent between military and civil. And the fact remains, but is not given due weight, that even though most early emigrants to America from Extremadura were artisans, they were drawn especially to Peru, chiefly through the example of, and recruitment by, its conquistador Francisco Pizarro. In addition, whether they were long-time residents or had arrived recently within a record migration to that Castilian frontier region and were again moving on, they were children of generations of civil strife and of a more recent war against Muslim Granada waged within a tradition of religious advance. For them the Indies were the current Spanish frontier, holding out, through the mixed military and civil endeavor long associated with migration, advancement of all sorts and also promising a "they" to give them not only labor but also an enhanced sense of "we" and a leg up from the bottom of the human social pyramid. It is, moreover, a pity that so little is said of diversity within Spain and of what is known of the presence in Extremadura of Moriscos and conversos and the probability that some of them were among the emigrants.

The impulse behind both books and the perspective informing them is American, having to do chiefly with immigration rather than emigration. Often missing are satisfying explorations of the outlooks of the emigrants and the dynamics of their European societies interpreted on their own terms rather than with a view to revising American history. It is all well and good to rediscover greater America and its social diversity, yet in general these studies construe social history rather narrowly and particularly pay too little attention to religious and political attitudes. Most of these studies scant cultural context and fall short of a stated intention to assess the significant impact of the emigration they address, either on Europe or on America.

PEGGY K. LISS
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A. J. R. RUSSELL-WOOD. *A World on the Move: The Portuguese in Africa, Asia, and America 1415-1808*. New York: St. Martin's. 1992. Pp. xv, 289. \$39.95.

As a counterpoise to Spain's recent efforts to trumpet the achievements of Christopher Columbus, the Portuguese government created the Comissão Nacional para as Comemorações dos Descobrimentos Portugueses to encourage books that would emphasize Portugal's achievements in the world beyond Europe. This volume is one of the works that the Commission supported. Its author, A. J. R. Russell-Wood, a well-known and widely published Brazilianist, offers seven essays, some previously presented as conference papers. They span the centuries from the fall of Ceuta in 1415, the traditional date for the founding of the Portuguese empire, until some time during the nineteenth century, for the author does not adhere closely

to his announced terminal date. After an initial essay that provides a quick overview of where the Portuguese pioneered and carved out a global thalassocracy and how they lost most of it, succeeding chapters examine various means of transport that the Portuguese and subject peoples employed; the kinds of people who voluntarily or involuntarily emigrated from the kingdom and the empire to other parts of the thalassocracy; the movement of bullion, cereals, cloth, dyestuffs, spices, sugar, tobacco, and other commodities within and beyond the empire; the global spread of flora, fauna, and disease, some by Portuguese hands but most by persons unknown; the transmission of Portuguese and non-European artistic forms, language, and ideas; and a review of early historical accounts of Portuguese exploits overseas. Based on wide reading in published, especially secondary, works, these somewhat repetitive, fact-packed essays present an extremely enthusiastic view of Portuguese achievements and tend to minimize their shortcomings. One can easily applaud Russell-Wood's enthusiasm for his subjects, but it is hard to believe that most specialists would agree that "the Portuguese experienced a series of 'encounters' . . . in Africa, Asia, . . . [and] in America, whose complexity and variety would make the Spanish experience in the Americas and the Philippines pale by comparison" (p. 10).

The text is enriched by the inclusion of eighty-four imaginatively selected and nicely reproduced illustrations, and by nine maps. One of the latter, a keyed endpaper, graphically presents the movement of commodities between Asia, Africa, the Americas, and Europe by the Portuguese and their rivals. But one map is misleadingly captioned and another contains puzzling numeration unexplained in the text.

One has the impression that these essays were hurried into publication. Otherwise, it is hard to comprehend some of Russell-Wood's flawed assertions. For example, Portugal did not cede Bombay to Britain in 1665 (p. 24); São Luis do Maranhão was not established in northern Brazil as early as 1595, nor were there Portuguese ports in southern Brazil as early as the mid-1580s (pp. 25, 140); the Portuguese adventurer Aleixo Garcia neither brought back nor sent back silver specimens from the Incan empire (pp. 14, 99); and the Society of Jesus was not founded in 1534 and its overseas provinces never played a "very prominent" role in bullion remittances to Lisbon (pp. 198, 146). It is difficult to know what to make of the author's statements that Brazil "was remarkable in that the expulsion of the Society of Jesus was attributable not to indigenous opposition or to the contrary forces of rival religions, but to the Portuguese themselves," or that after the expulsion, "efforts were made to improve education" (pp. 202, 208). Indigenous peoples and non-Catholics were hardly consulted anywhere by those who engineered the definitive expulsion of the Jesuits, whose removal from the Portuguese empire was certainly not fol-

lowed by significant educational innovations. Nevertheless, this volume provides a useful introduction for nonspecialists concerning the role that the Portuguese played (or were believed to have played) overseas during the early modern period.

DAURIL ALDEN
University of Washington

G. R. EVANS. *Problems of Authority in the Reformation Debates*. New York: Cambridge University Press. 1992. Pp. xv, 328. \$59.95.

G. R. Evans has selected a splendid topic for exploring the disagreements of the sixteenth century. The issue of authority—of a text (like the Bible) and its authoritative interpretation, of individuals (such as the pope or the church fathers) and institutions (for example, the hierarchical church or local congregations), and of reason and specific forms of “proof” (for instance, arguments accepted in disputations)—underlay sixteenth-century disputes about sin and justification, the power of the sacraments (especially baptism, penance, and the eucharist), authority in the “visible community” (including political rulers, priests, bishops, and the pope), and who ultimately had the power to decide the many issues in dispute. Evans examines each of these major areas.

What Evans offers is not a conventional history of Reformation debates over authority, but rather a study in ecumenical theology that looks to these earlier debates for insight. This ecumenical venture proceeds in a fashion rather out of style in current historiography, with an odd mixture of historical theology leavened by normative theological judgments. This mixture is likely to be jarring to historians, especially if they do not share Evans's theological convictions. To give one example among a wealth of controversial theological (and historical) judgments, she concludes that Martin Luther's understanding of justification “failed to meet the difficulties about powerlessness created by sin. It did so first in not containing a full account of the nature of the restoration of the sinner to the full working order of the human nature in which he was created; and secondly in not fully relating the salvation of the individual to the salvation of the community with which he is united when he becomes Christ's” (p. 134). Since it is probably inappropriate to enter into a theological debate here, suffice it to note that Evans, while attempting to be even-handed, tilts rather heavily in the direction of Roman Catholicism on many controversial issues then and now.

There are also serious shortcomings in the way she approaches the historical part of her study. She limits her consideration, for example, largely to formal confessional documents and disputation articles, a practice that is hard to understand methodologically and may account for various misstatements concerning the views held by Luther, John Calvin, and

various of their Catholic opponents. Had she, for example, consulted Luther's *On the Councils and the Church* (1539), she might have avoided several flat-footed statements about Luther's views on both conciliar authority and the church. More serious is her decision to eschew any serious attempt to place the various theological positions within a social, political, or institutional context. This allows Evans, for example, to make normative judgments about episcopacy and “headship on earth” (for example, p. 231) that pay far too little attention to the ways episcopacy and the papacy actually functioned in the sixteenth century (or, for that matter, the ways in which they function today). There is a certain disembodied or theoretical quality to much of her analysis, as if theology could be disconnected from real institutions and from considerations of power and self-interest.

Evans shows great learning in the history of Christian theology and a commendable desire, in a worthwhile cause, to learn from the “mistakes” of the past. Unfortunately, I doubt that historians will find the work very useful. Although more limited in scope, David Bagchi's *Luther's Earliest Opponents: Catholic Controversialists, 1518–1525* (1991) does a far better job exploring the issue of authority in the controversial writings of the early Reformation.

MARK U. EDWARDS, JR.
Harvard University

CARLO M. CIPOLLA. *Miasmas and Disease: Public Health and the Environment in the Pre-Industrial Age*. Translated by ELIZABETH POTTER. New Haven: Yale University Press. 1992. Pp. ix, 101. \$20.00.

In recent years the works of Ann Carmichael, Carlo M. Cipolla, Katharine Parks, and Nancy Siraisi have strengthened our understanding of plague and other diseases in medieval and Renaissance Tuscany. Cipolla has provided a particularly bountiful harvest in four works covering the history of public health in the late Renaissance. To me, Cipolla's most important epidemiological studies are *Public Health and the Medical Profession in the Renaissance* (1973) and *Faith, Reason and Plague in Seventeenth-Century Tuscany* (1987). The former still stands as a point of departure for further study; the latter, which treats popular and religious responses to Florentine public health regulations in the tiny Tuscan *castello* of Montelupo, is a classic of the micro-historical genre.

The work under review was originally published in Bologna in 1989 as *Miasmi ed Umori*; with a revised title, it now reaches English-language audiences in a skillful translation by Elizabeth Potter. The book is short (only eighty pages of text), and it is restricted in time and place to the first three decades of seventeenth-century Tuscany. It is also narrowly focused on what the officials and contacts of the Florentine public health magistracy, the *Magistrato alla Sanità*, thought and said during periods of epidemic about

sick people, disease, and the relationship of both to environmental conditions in numerous locales throughout the Florentine Grand Duchy.

The book has five chapters. The first surveys the genesis and operation of the *Sanità*, summarizes and analyzes the contemporary humoric-miasmic plague paradigm, and introduces miasma, regarded as the chief vector of plague and defined as corrupt and infectious air normally generated by odors derived from waste of all sorts. Cipolla details the latter in chapter 2, citing reports of sanitary conditions sent to Florence in 1622 by officials of thirty-seven towns, villages, and *castelli* of the Grand Duchy in response to an ordinance issued by the *Sanità*. A veritable census of filth, rubbish, and stink poured in, emphasizing that excremental stench, both human and animal, was endemic everywhere. Odors from slaughterhouses, tanneries, stagnant water, and insufficiently buried corpses all exacerbated the fecal pollution. Cipolla's most thorough discussion occurs in chapter 3, tracing the steps taken by *Sanità* doctors and local officials to monitor various epidemics between 1608 and 1623, the concern always being to discover whether plague was present. It was not, but the medical and official reports sent to Florence, quoted extensively, reveal a wealth of contemporary epidemiological information regarding identification of particular diseases, application of medical jargon and treatment methodologies pertinent to them, and estimation of their causations. The book concludes with two short chapters containing brief reflections on some of the matters raised. An appendix provides an English translation of the ordinance of 1622, while notes supply adequate references and supplementary information. A glossary renders definitions of contemporary terms, not all of them medical.

The importance of this book lies in its attention to contemporary paradigms that attributed illness to environmental pollution. Modern scholars are generally aware that these paradigms existed, yet little has been done to mine the sources for evidence indicating that they were known and believed beyond the medical community. Cipolla confronts the issue, however, showing that the *Sanità* documents, which included the opinions of physicians as well as local officials and people from all levels of clerical and lay society, often attributed epidemics to malevolent odors generated by waste, filth, and stagnancy. The vital subject of preindustrial epidemiology is thus well served, for Cipolla's findings invite careful search for similar evidence in the documents of other Renaissance times and places.

JAN T. HALLENBECK
Ohio Wesleyan University

JEAN DIETZ MOSS. *Novelties in the Heavens: Rhetoric and Science in the Copernican Controversy*. Chicago: University of Chicago Press. 1993. Pp. xiv, 353. Cloth \$49.95, paper \$17.95.

Considerable attention has been devoted of late to the rhetoric of science; the University of Pittsburgh recently inaugurated a program in the rhetoric of science, and a number of books have joined an increasing flood of articles on the subject. Jean Dietz Moss offers a book that invites us to take a deep breath before jumping into that flood.

Her central point, a good one, concerns the referent of the term "rhetoric." She notes that the classical origins and development of the term placed it in a particular relation to two others, "demonstration" and "dialectic." The first denotes the production of certainty, the second the production of the most likely conclusion. "Rhetoric," by contrast, denotes the production of localized credence and must always be understood in relation to a particular audience. In discussing the role of rhetoric in early modern astronomical texts, Moss adheres rigidly to this trichotomy in an attempt to distinguish each of the three modes of argument and the work each does in establishing a work's assertions. The intention is to avoid the loose usage of the term "rhetoric" that nowadays dominates to the disadvantage of more nuanced textual analyses.

Moss's particular approach, however, contains some difficulties. Her overall claim is that rhetoric once played only an ornamental role in scientific writings, the real argumentation being demonstrative and dialectical. Thus, Nikolaus Copernicus's *De revolutionibus* (1543) adhered closely to such forms, largely eschewing rhetoric except for such formal exercises as its dedicatory letter. With Galileo's arrival on the scene, rhetoric begins to play a significant part in winning assent owing to his appeal to a wider audience than that of academic insiders who know the rules of proper demonstrative and dialectical argument.

Despite Moss's claim, especially in her postscript, that she means to be purely descriptive, leaving philosophical issues for those whose tastes run to them, the entire tenor of her book underwrites the methodological judgments of the historical actors. Galileo's departures from good dialectical argument in favor of rhetoric are treated as questionable moves in a scientific game whose legitimacy derived from its adherence to the Aristotelian codes dominant at the time, rather than as novel attempts at restructuring what counted as dialectic and rhetoric. Moss notes on a couple of occasions the Ramist reformation of logic and rhetoric in the sixteenth century, observing its adjustment of categories so as to leave to rhetoric only the parts concerning theatrical issues of style and moving *inventio* and *dispositio* to logic. She does this in order to stress the distinct role of logic in argument and its sharp distinction from rhetoric. But it could equally well be argued that this Ramist move underlines the fluidity of the categories in the period and the lack of clarity surrounding rhetoric's boundaries and integrity.

With these structural characteristics, the book provides paraphrases of varying levels of detail of a

number of signal texts in the early history of Copernicanism. Its central focus is on Galileo; Moss stresses especially Galileo's familiarity with the appropriate categories of argument from his early logical writings. Galileo's appeals to an extra-academic readership in his controversies on sunspots and comets as well as in the *Sidereus nuncius* (1610) and *Dialogo* (1632) help to explain the substantive role of rhetoric as contrasted with Copernicus or Johannes Kepler. One aspect of Moss's account, however, is troublesome: her characterization of the important category of mixed mathematics in this period. She portrays mixed sciences such as astronomy as if they were regarded as imperfect mixtures of mathematics and physics. But mixed mathematical sciences were seen as integral parts of mathematics. They were usually clearly distinguished from physics, eschewing as they did physical causes.

The book ends with a discussion of John Wilkins's popular books of 1638 and 1640 on the heavens, *The Discovery of a World in the Moon* and *A Discourse Concerning a New Planet*. Since Moss wants to portray Galileo as having introduced rhetoric into scientific argumentation, inaugurating a rhetorical revolution, this is a surprising choice, since Wilkins stays safely clear of rhetorical grandstanding. Wilkins acts as a fine exemplar, however, of the kind of argumentative strategies Moss uses as her benchmark of Renaissance sensibilities in the evaluation of proper procedure in the understanding of nature. As a book detailing what one ought to expect to find in philosophical texts of this period, and how to read their possible departures from it, this presents a useful model.

PETER DEAR
Cornell University

A. RUPERT HALL. *Isaac Newton: Adventurer in Thought*. (Blackwell Science Biographies.) Cambridge, Mass.: Blackwell. 1992. Pp. xv, 468. \$29.95.

Few scholars are better equipped to write a general biography of a great thinker than A. Rupert Hall, the noted English historian of science. An editor of the Newton correspondence as well as that of Henry Oldenburg, the first secretary of the Royal Society, Hall has also edited some of Newton's unpublished papers, thus acquiring an easy familiarity with the documents that only a lifetime's research can provide. At the same time, specialists used to writing almost exclusively for their peers often have difficulty relating to a lay audience, unfortunately the case in this instance.

In his foreword Hall attacks Frank E. Manuel's Freudian approach to Newton (*A Portrait of Isaac Newton* [1968]), deeming it imprudent to interpret the natural philosopher's life and writings in terms of single factors. This is certainly fair enough. Yet one searches this volume in vain for Hall's interpretation of Newton, the *sine qua non* of biographical writing. One of many examples must suffice. Famous for his

temper and professional jealousy, Newton is excused by Hall at one point on the grounds that, in making such judgments, modern scholars "[p]erhaps ignore differences in styles of rhetoric between Newton's age and our own" (p. 140). Yet twenty pages later he writes that Newton's "mildly paranoid tirades" (p. 160) first surfaced in childhood, when he threatened to burn the house down with his mother and stepfather still inside. As every student of Newton is aware, that "mild paranoia" became all-consuming fury when Newton later tangled with Robert Hooke, John Flamsteed, and Gottfried Wilhelm Leibniz over issues both real and imagined.

More disappointing is the manner in which this largely intellectual biography manages to stifle the thrill and magnificence of the discoveries that came to define a historical age. Hall plods through the mathematics and optics in great detail, often with little regard for chronology (late and early work are mingled), then apologizes for not including still more equations, the fear of criticism by fellow scholars a constant concern. In forsaking the general reader, who will be incapable of following many of the arguments, Hall will not have pleased the specialist either.

Nor is the material formed into an integrated whole. Instead, chapter subtitles, reading like the infamous laundry list, are dumped at the reader's doorstep in the hope that he or she will make the connections: "Newton Responds to Critics," "Mathematical Correspondence," "The Colours of Thin Plates," "Particles and Light," "Newton's Aetherial Hypothesis, 1675," and so on. One soon begins to think of this work as a pastiche, a hodgepodge lacking definition. So devoid is the work of life that Hall felt it necessary to gather some of the more interesting material into a series of appendixes on such subjects as Newton's portraits, his residences in London, and the fascinating relationship between his live-in niece Catherine Barton and her literary friend Jonathan Swift.

A final cause of frustration is the prose itself, which, as the following sentence attests, is often difficult if not impossible to follow: "Not surprisingly, the paradoxical notion of a 'force of inactivity,' a force that is not a dynamic force, has occasioned great difficulty to commentators then and since, particularly when linked with the idea of the relativity of local motion (powerfully exploited by Huygens) as it must be, since in the discussion of local events, as distinct from cosmological events, for the sake of symmetry the description must be independent of the observer's subjective sense of being in motion or at rest" (p. 56). Hardly what Catherine Drinker Bowen (*Adventures of a Biographer* [1959]) had in mind when she noted that her primary concern as a biographer was to keep the reader turning the page.

GALE E. CHRISTIANSON
Indiana State University

INGO WILHELM MÜLLER. *Iatromechanische Theorie und ärztliche Praxis im Vergleich zur galenistischen Medizin (Friedrich Hoffmann–Pieter van Foreest, Jan van Heurne)*. (Historische Forschungen, number 17.) Stuttgart: Franz Steiner. 1991. Pp. 320. DM 72.

Friedrich Hoffmann (1660–1742) was one of the most prominent physicians and medical professors at the turn of the eighteenth century, but he has not received the same attention from historians as have his contemporaries Georg Ernst Stahl and Hermann Boerhaave. Older intellectual histories that trace the rise of modern medicine discussed Hoffmann as a transitional figure whose only innovation was to clothe the older Galenic medicine in the modern garb of seventeenth-century mechanist principles. Recent work in the history of medicine has been more concerned to situate medical ideas in their historical context, but in its attempt to move away from "Great Doctor" historiography it has focused on patients rather than physicians and students rather than professors. Ingo Wilhelm Müller's book is thus welcome: it provides a comparison between Hoffmann and his Galenist predecessors while insisting that they must be understood in their own terms rather than as forerunners of modern medicine.

Müller's analysis is detailed but narrow. He does not treat all of Hoffmann's ideas, focusing instead on pleurisy and pneumonia. His comparison is based on six cases found in Hoffmann's major work, *Medicina rationalis systematicae*, and twenty-two cases taken from Pieter van Foreest's *Observationum et curationum medicinalium ac chirurgicarum* (1634), supplemented by Jan van Heurne's textbook *Opera omnia* (1658). Müller compares and contrasts their ideas on diagnosis, physiology, pathology, and therapeutics. The latter two sections cover every symptom from fever to deficiency in the nervous fluid, every remote cause from age to plethora, and every form of treatment from bloodletting to diet. Anyone interested in a detailed analysis of pleurisy and pneumonia in the seventeenth and eighteenth centuries need look no further.

Müller's conclusion, that Hoffmann retained much of Galenist practice while rejecting all Galenist explanations in favor of mechanist principles, does not contradict those of earlier historians. But they criticized Hoffmann for not discarding Galenism as excess baggage; in contrast, Müller notes that physicians in Hoffmann's day faced the intellectual challenge of explaining why, if the new science was correct, the old medical practice had worked so well. Medicine was not astronomy: Johannes Kepler's ellipses led to better star charts than Ptolemy's epicycles, but William Harvey's circulation did not lead to more cures for pneumonia than Galen's souls. For that reason Hoffmann had to take Galenic practice seriously.

Müller also argues convincingly that Hoffmann's reliance on mechanist principles was not mere window dressing but instead a fundamental distinction

between his ideas and those of van Foreest and van Heurne. Hoffmann's belief that the immediate cause of disease lay in the obstruction of circulation determined his evaluation of post-mortem dissection; it also led him to reject the four humors while retaining the concept of different temperaments as a fact well known to all experienced physicians. Müller's larger argument is that for Hoffmann, experience was the yardstick by which all theory should be measured. His iatromechanism should not be portrayed as an impractical theory, because for Hoffmann a theory was only valuable insofar as it was practical.

The narrow scope of this book does not allow us to extrapolate Müller's conclusions to other iatromechanists, other Galenists, or even other diseases. Taken on its own terms, however, it is successful and will be of interest to historians of early modern medicine and science.

LISA ROSNER
Stockton State College

DAVID KNIGHT. *Humphry Davy, Science and Power*. Cambridge, Mass.: Blackwell. 1992. Pp. xiii, 218. \$29.95.

Humphry Davy set out to be a second Isaac Newton, with chemistry as his domain instead of physics; if he did not quite achieve that exalted rank, he did succeed in becoming the preeminent British scientist of his day. David Knight's engaging biography presents a vivid account of Davy's life, his science, and his cultural milieu. Previous biographies, according to Knight, were written as though Davy's early scientific triumphs were the high point of his life, with his poetry, his religious writings, and even his tenure as president of the Royal Society seen as anticlimactic if not irrelevant. By contrast, Knight shows Davy moving in a natural progression from creative scientist and brilliant lecturer to administrator and then to philosopher, never ceasing to ponder, even in his later contemplative writings, the ideas elaborated in his early work on electrochemistry.

The most stimulating parts of the book are based closely on Davy's own writings, and the most penetrating have to do with his science. Knight does a masterful job of presenting both the issues at stake in early nineteenth-century chemistry and the solutions Davy found for them. Equally skillful is his discussion of the two factors that contributed most to Davy's fame in his own lifetime, his public lectures and his safety lamp. When the book moves away from Davy's own words, as in the chapter on his presidency of the Royal Society, it loses momentum. Davy was widely considered to be a failure as president, in part, as Knight shows, because he set himself an impossible task, the reformation of the Royal Society from a coterie of well-born amateurs to an elite of professional scientists. This is a complicated topic, and Knight perhaps set himself an impossible task in

discussing it in a single chapter, especially since Davy's own perspective is not very illuminating here.

Knight observes that "biography is a dialogue, in which one participant is usually (and perhaps better) dead" (p. xii), and he does, indeed, carry on a running conversation with and about Davy, informing the reader, for example, when Davy's own view of the world should be taken with a grain of salt. Specialists will recognize that Knight is carrying on a dialogue with them as well, about ideas and experiments in the history of chemistry, about the limits of the concept of "normal science" as explanation of scientific activity, about the relationship between teaching and research, about patronage in science. But the book is too short for much elaboration of these ideas, and interested readers will find themselves having to fill in gaps in both sides of the conversation.

This book is one of a series of short biographies of scientists and succeeds well in its goal of presenting an accessible account of both Davy and his times. Scholars will benefit from the discussion of Davy's science but are likely to be disappointed in the sketchy notes and bibliography. A lengthier, more nuanced biography of Davy, with full scholarly apparatus, would be most welcome.

LISA ROSNER
Stockton State College

ANGUS FRASER. *The Gypsies*. Cambridge, Mass.: Blackwell. 1992. Pp. ix, 359. \$24.95.

This book is included in a series about the peoples of Europe "from their origins in prehistory to the present day." Five maps track Europe's Roma or Gypsies from their ancestral lands in the Indian subcontinent to deportation routes that brought them to Nazi extermination camps. Forty-eight plates present images from fifteenth-century drawings and tapestries depicting Gypsies long settled in Europe to a photograph of a Romanian Gypsy being arrested in Bremen in 1990. Angus Fraser points out that his task is made difficult by the fact that the Roma are a people of Europe "without a common language, a common culture and a common racial type" whose "main achievement is to have survived at all" (p. 1). His study looks back to their ethnic origins in India, traces their cultural and linguistic fragmentation across Europe, reviews laws passed to expel or exterminate Europe's dark-skinned "nuisance" (p. 249), and ends with recent attempts by pan-Gypsy political organizations to assert demands for basic human rights and an end to "perceived injustice by the host society" (p. 316).

In the first three chapters, Fraser reviews linguistic evidence and debates that point to the Indic origins of proto-Romani. He draws on studies that date the passage of the Roma from Persia into Greek-speaking territories in Asia Minor following the Arab conquest

of Persia in the seventh century, and from Asia Minor into Byzantium's European provinces after the Seljuk conquest of Anatolia in the eleventh century (when the name *Atsinganoi* came into use). Historical records next track Europe's first "black people" into southeast Europe in the fifteenth century, moving ahead of the Ottoman Turks. Here Fraser directs his study toward the "Egyptians"—the name already in use "as a contemptuous insult" (p. 48)—migrating into central Europe. He mentions, but does not elaborate on, the experience of Europe's Gypsies in four centuries of slavery in the Romanian provinces of Wallachia and Moldavia.

In chapter 4, "The Great Trick," Fraser makes much of historical documents that record the granting of numerous letters of safe conduct to exotic, self-proclaimed "penitents" and pilgrims (p. 63) from Egypt as they moved through Catholic Europe, and in the next chapter, he scans sixteenth-century sources that reveal to what extent the already stereotyped (often described as "ugly" [p. 67]) pilgrims had worn out their welcome in post-Reformation Europe. In chapter 6, "The Pressure of the Gyves," Fraser surveys repressive legislation across Europe in the seventeenth and eighteenth centuries aimed now at annihilating, now at assimilating, now at transporting Gypsy groups to Africa or to the New World. The Roma emerge as survivors precisely at the time European philologists first linked the Gypsies' way of life and language to Aryan nomads who had settled in mainland Greece centuries earlier.

We are reminded of the Gypsies' "natural performing ability" (p. 201) in chapter 7, and the significant impact of their performers on various Hungarian, Russian, and Spanish musical styles. In 1867 the largest concentration of Gypsies in Europe finally gained legal freedom in Romania. What joined them to their distant kinsmen in Western Europe was not their language (or languages), Fraser finds, but funerary customs and pollution beliefs that were a "core element of their cultures, serving to express an ethnic boundary and delineate a fundamental division between Gypsy and *gadzo*" (p. 245). In chapter 9 the author moves from nineteenth-century European romantic nationalism to twentieth-century racism, the end result of which was the attempted extermination of one Aryan group in Europe by another in the "forgotten holocaust" (p. 257). Here Fraser provides helpful references to recent works that more narrowly focus on the destiny of the Roma outside Protestant and Catholic Europe and the atrocities of the Nazi period. His study returns to the diversity of dialects, religious affiliations, and lifestyles of Roma groups thrown together by relocations resulting from the recent political and economic upheavals in Europe. In a united Europe, Fraser concludes, unity for a pan-European people "is not likely to come easily" (p. 318).

Fraser's volume satisfies the expectations of the editors of the series on the peoples of Europe. Al-

though the linguistic and historical data he presents may at times seem to skip across the current of the Gypsy experience in Europe, Fraser's highly readable account nevertheless points the reader toward still uncharted depths.

JOHN KOLSTI
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JOHN J. MCCUSKER and CORA GRAVESTEIJN. *The Beginnings of Commercial and Financial Journalism: The Commodity Price Currents, Exchange Rate Currents, and Money Currents of Early Modern Europe*. (NEHA, third series, number 11.) Amsterdam: NEHA. 1991. Pp. 515.

After more than a decade of Herculean labors, the American and Dutch scholars John J. McCusker and Cora Gravesteijn have rendered an inestimable service with this inventory of newspaper sources in early modern price history from documents—by their nature ephemeral—found in archives and libraries in seventeen countries. The term “current” in the title refers to those commercial publications in newspaper format, often officially sanctioned, that reported commodity prices, currency-exchange rates in negotiable bills, and coinage-exchange rates from the most current international-market transactions. The book is organized alphabetically, with chapters for each of the thirty-three European towns and fairs, from Amsterdam to Vienna, whose prices and exchange rates have been so recorded. The earliest currents date back to around 1540, with available information increasing with the diffusion of the printing press; this inventory concludes with 1775.

Commodity-price currents are vastly preferable to prices gleaned from institutional accounts, the data on which historians have so far primarily relied. Too often these lacked consistency or homogeneity and involved long-term contractual purchases that imperfectly reflected the market. Furthermore, as McCusker and Gravesteijn aptly remark, “business thrives on the most recent news” (p. 21). Since early modern business firms often transmitted published information by mail to agents and clients across Europe, currents also provide much valuable information on how commerce and finance were conducted during the era of the commercial revolution.

This volume of dazzling scholarship has already elicited praise from one of the leading authorities on eighteenth-century financial history, Larry Neal (in the *Journal of Economic History* 52: 3 [1992]). To reiterate its numerous virtues here would needlessly consume space better used to explain its limitations, especially those imposed by the sources themselves. First, this book is not about commercial journalism, although it often provides valuable information, in both the text and voluminous footnotes, about commerce, finance, and the methods of publishing their currents in the towns covered. Second, contrary to

any implications in the subtitle, the book is not a companion to, say, Peter Spufford's *Handbook of Medieval Exchange* (1986); it does not present any tables of commodity prices or exchange rates, except for some photographic (and difficult to read) reproductions of selected documents. For such tabulated data, the reader should instead consult McCusker's earlier and even more valuable book *Money and Exchange in Europe and America, 1600–1775* (1978). Third, the extant exchange-rate currents greatly outnumber those for commodity prices, and there are very few coinage-rate currents, with listings for only four (German) towns. Fourth, the documentary sources indicate that long-run commodity-price (CP) or exchange-rate (ER) series, with ten years or more of consecutive data (often with single-year gaps), exist for only five towns: Amsterdam (CP: 1624–54, 1668–79, and 1686–1775; ER: 1691–1705); Antwerp (ER: 1665–80, 1688–1714); Bologna (ER: 1674–86); Genoa's Santa Margherita fair (ER: 1712–22); London (CP: 1674–1704; ER: 1698–1775, and 1747–75 with insurance rates); and Venice (ER: 1627–86, 1764–75). For early modern commerce the Amsterdam data are clearly the most important, but most of these commodity prices have already been tabulated by Nicolaas Posthumus in his *Inquiry into the History of Prices in Holland* (2 vols., 1946–64). The extremely valuable exchange-rate data for London are already available on microfilm from the University of London, although evidently the Venetian exchange-rate data are not so accessible. Nevertheless, lest these observations seem unduly churlish, I must concur with Neal's verdict that McCusker and Gravesteijn “may well have launched a new era in quantitative price history” (p. 708) with their exhaustive inventory of these commercial and financial currents.

JOHN H. MUNRO
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DAVID KAISER. *Politics and War: European Conflict from Philip II to Hitler*. Paperback edition. Cambridge: Harvard University Press. 1992. Pp. 435. \$14.95.

David Kaiser has written this book to offer new perspectives on an old problem: the causes and consequences of war in European history. He identifies four distinct periods of general conflict when “war became a natural function of politics, an inevitable result of contemporary political behavior” (p. 1). In his first period, stretching from 1559 to 1659, recurring struggles grew out of attempts by kings and princes to assert their authority over great aristocratic families and maintain religious uniformity throughout their lands. During the age of Louis XIV (1661–1713), their successors used firmer control over the instruments of war to pursue dynastic claims and promote the commercial interests of their domains. Kaiser sees events in the revolutionary and Napoleonic period (1792–1815) as driven by military expan-

sion aimed at the consolidation of central authority and a more rational definition of the Continent's political map. Finally, in the era of the two world wars (1914–45), governments fought partly to achieve economic self-sufficiency for their states and partly to advance the principle of nationality. Kaiser associates each of these periods with unique developments shared by all of the significant European powers, whose similarities typically turned armed conflict into a series of indecisive tragedies. "Fighting has a logic of its own," Kaiser laments, "and the logic which governs political behavior need not seem reasonable or sensible when evaluated according to its consequences" (p. 3).

Kaiser's deterministic assumptions, perhaps an outgrowth of the late-Cold War political environment, yield mixed results. By adroitly synthesizing substantial bodies of recent scholarship, he has fulfilled his promise to acquaint readers with basic information about modern European history. His skillful treatment of topics like the expansionist tendencies of revolutionary France as facets of broader Continental patterns produces insightful explanations. Yet many of his conclusions are not original: the idea that Napoleon embodied the spirit of his age, the assertion that World War I made demands on governments that could not be met, or the thesis that war has been the supreme test of modern states. Moreover, Kaiser must occasionally be selective with his evidence to sustain his interpretation. A striking exaggeration of Richelieu's failures coupled with an overly generous assessment of Louis XIV's action is a notable instance of this propensity.

Kaiser has trouble explaining the shifts from one phase of general conflict to another. He overestimates the enhancement of royal authority during the reign of Louis XIV, thereby distorting the political patterns of the early modern period. His emphasis on the eighteenth-century intellectual climate, understood as substituting the idea of reason for traditional practices and dynastic rights, cannot by itself explain politics and war in the revolutionary epoch. Kaiser's problems with transitions are compounded by his preoccupation with the role of prominent individuals in the political process. Especially disconcerting is his picture of the Sun King, who appears in this volume as ever rational, selfless, and dedicated to prudent objectives. Kaiser's substantial accomplishments are further compromised by his cavalier dismissal of Europe's mid-eighteenth-century wars and by his total neglect of the myriad disruptive struggles coinciding with the Hundred Years' War as well as the early sixteenth-century confrontations between Habsburg and Valois (1521–59), which began just as the Ottoman Turks were moving into Hungary and not long before Charles V provoked the Schmalkaldic War with the territorial princes of Germany. Ironically, these periods of widespread upheaval reinforce Kaiser's contention that war has not been an effective means of realizing political ambition, but they also

serve as reminders of the significant gaps in his basic argument.

JOHN A. MEARS
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WILLIAM HENRY FLAYHART III. *Counterpoint to Trafalgar: The Anglo-Russian Invasion of Naples, 1805–1806*. Columbia: University of South Carolina Press. 1992. Pp. xi, 198.

William Henry Flayhart III has produced a delightful little book for those who prefer old-fashioned military-diplomatic history about a little-known campaign of the Napoleonic Wars. Fearing another French offensive into the eastern Mediterranean, Russia and Britain agreed to mount a joint effort to save the Kingdom of the Two Sicilies to prevent that possibility. The background preparations for this ambitious and ill-fated venture are described in fascinating detail, especially on the British side, while the invasion itself is treated with deserved brevity, since nothing was really accomplished. The Russian and British force was too small and ill-equipped to stop any serious French attack on Naples, and the outcome of the Battle of Austerlitz caused Alexander I to order his troops withdrawn in January 1806.

The story has been told before in the larger context, notably by Piers Mackesy (*The War in the Mediterranean, 1803–1810* [1957]), but it deserved its own treatment and interpretation. Flayhart is especially skilled at sketching the personalities of the generals, diplomats, and politicians involved in planning and carrying out the enterprise. General James Craig, afflicted by heart disease, is the hero on the British side, while the leaders of the Russian forces, Maurice Lacy and Roman Anrep, are given more critical and less extensive attention. One of the weaknesses of the concentration on the British participation is the uncertainty regarding Russian motivation and actions. Much work in Russian on this subject by Evgenyi Tarle, Aleksandr Shapiro, Avgusta Stanislavskia, and Vladlen Sirotkin is absent here. Much of the Russian documentation, moreover, is available only in the original language (*Vneshniaia Politika Rossii XIX i Nachala XX veka*, series 1, volumes 2 and 3 [1960]).

Flayhart's thesis that the Anglo-Russian Mediterranean campaign of 1805 was a direct cause of Nelson's victory at Trafalgar is interesting but can be easily challenged. It was an important factor in drawing the French fleet into open water in October 1805, but other events, Napoleon's recklessness, or simple boredom might have done the same. Similarly, the contention that British forces on Malta and Sicily effectively blocked French movement into the eastern Mediterranean ignores the French acquisition of enclaves along the Dalmatian coast by the Treaty of Pressburg and the Russian Adriatic campaign of Admiral Dmitri Seniavin in 1806 to contain this threat. Nevertheless, despite the limited scope, this

well-written and organized study belongs in the library of every Napoleonic and military history buff and of scholarly institutions.

NORMAN E. SAUL
University of Kansas

JOHN R. GILLIS *et al.*, editors. *The European Experience of Declining Fertility, 1850–1970: The Quiet Revolution*. (Studies in Social Discontinuity.) Cambridge, Mass.: Blackwell. 1992. Pp. xii, 385.

In 1963, Ansley J. Coale launched a study at Princeton University of the European fertility decline; the concluding volume was published in 1986. The project provided indexes of fertility and of the proportion of the population that was married computed over time for 700 province-sized administrative units in Europe for which there existed census and vital registration data. On this evidence, the prevailing theory of the demographic transition, which linked fertility decline with structural characteristics such as industrialization and urbanization, was found wanting. The task of the Princeton team was exploratory rather than a search for grand theories. It proposed empirical generalizations of a descriptive nature: for example, there was a plateau of high marital fertility in most provincial series where the data went back far enough; there was a clear, datable break in each series; once started, the decline was continuous until fertility reached a much lower level. The team's strongest theoretical statement is known as "Coale's three pre-conditions": for a fertility decline to occur in a population, there must be an awareness of reproductive options, motivation to limit births within marriage, and access to means of control.

The present volume acknowledges its place in the post-Princeton project era. It begins with a useful guide to theories of fertility decline by George Alter and ends with a number of chapters that look at the consequences of fertility decline or treat it as background. I will concentrate here on the central chapters that decipher particular episodes of the transition. The authors of this volume use sources other than the data of the European project: direct interviews (Jane Schneider and Peter Schneider for a rural Sicilian town, Martine Segalen for two regions of Brittany), published autobiographies (Mary Jo Maynes), or a combination of testimonies collected by observers of the time (John R. Gillis on the British middle class, Ellen Ross and Angus McLaren on the British working classes). Wally Secombe uses letters of women in England and Norway and the results of German surveys on sexual behavior. It is remarkable that so much direct evidence on attitudes and couples' relations is coming to light. It captures the anguished voices of people confronting the prospect of excessive fertility with a limited arsenal of methods of contraception and abortion, a fascinating and necessary complement to the dry language of statistics.

The interpretation of this evidence is a different matter. The book is full of fascinating but largely unsubstantiated hypotheses about the timing of the fertility decline. Most of the contributors pay lip service to Coale's preconditions. They discuss the availability of means of family limitation. Economic motivations figure strongly in Secombe's, the Schneiders', and Segalen's articles; Segalen resorts to an explanation of fertility differences between two Breton *parishes* as a function of their tenure systems. The most popular explanations rely on a transformation of family and gender roles, which changed the perception of children's cost; the decline of fertility was conditioned by the acquisition of values of motherhood incompatible with large families. Gillis argues that this occurred among the English middle classes by 1850. Ross suggests that state intervention and the efforts of welfare activists to impose middle-class ideals discouraged working-class women from having babies. Secombe claims that the medicalization of women's health problems and the routine intervention of doctors in childbirth served as a catalyst. Chiara Saraceno writes that the Italian state imposed an ideology of domesticity that ultimately led to lower fertility. These theses are familiar and plausible, but the problem remains to account for the precise timing of fertility change in a testable way.

David Levine, in a postscript to the volume, opposes "the spurious specificity implied by the statistical manipulation of large-scale aggregations" (p. 331) to the historian's narrative that considers individual decisions in context. The discipline of history is broad enough to accommodate methodologies that allow us to see both the forest and the trees, the patterns on a European scale and local processes. A separate look at, say, the laws and society of fascist Italy provides a valuable case study but confirms the similarities with neighboring countries. Here and there contributors (such as Michael R. Haines and Secombe) attempt international comparisons and suggest interconnections and parallelisms. The volume contributes depth and color, but it should not underplay the fundamental unity of the European story of the fertility transition.

ETIENNE VAN DE WALLE
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HARVEY GOLDMAN. *Politics, Death, and the Devil: Self and Power in Max Weber and Thomas Mann*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xi, 386. \$35.00.

This book is a sequel to Harvey Goldman's previous work, *Max Weber and Thomas Mann: Calling and the Shaping of the Self* (1988), which analyzed the concepts of calling and personality in the writings of the two men. Focusing in his new work on the years after 1914, Goldman builds on many of his earlier themes: the parallels in Weber's and Mann's thinking; the

presence of a common ground between social thought and literature, making it possible to analyze them as a cohesive whole; and the secularized Protestant system of values that emerges from the comparison. In addition, however, his new volume strikes out in a different theoretical direction. Drawing on Michel Foucault and other recent thinkers, Goldman analyzes "the way in which Weber and Mann sought an antidote to personal and cultural weakness through practices for generating strength, mastery, and power" (p. ix). This approach succeeds in introducing a fresh vocabulary into the discussion of two of the most heavily discussed figures of twentieth-century intellectual history, linking their achievements and ideals to underlying moods of personal, class, and national vulnerability.

Faced with a weakening bourgeois German ideal of *Bildung* or self-cultivation, Weber and Mann sought new forms of personal discipline to shape the self into a creative actor. Weber, according to Goldman, looked back to the Puritans for a practice of ascetic individualism, while Mann tried to revive the relationship between artist and community. From 1910 until his death in 1920, Weber radicalized his search for techniques to shape a heroic self. After 1914, Weber's personal ideal of the calling consisted not just in devotion to one's work but also in a form of combat: "Weber's real task has become nothing less than to make the individual a soldier on the battlefield of the 'impersonal gods' of a calling" (p. 67). Weber dramatized the inner life of the individual as a devotion to one's chosen "god" and hostility to competing values.

This notion of soldierly service was supposed to invigorate scholarly activity, which Weber at the end of his life believed to be devoid of intrinsic meaning. In the realm of politics, Weber expected the charismatic leader to invigorate politics through a similarly single-minded devotion to a cause. Goldman takes issue with Weber's portrayal of William Ewart Gladstone and David Lloyd George as exemplary charismatic politicians, pointing out that both were more compromising tacticians than was compatible with Weber's ideal type. Goldman does less than justice to the critical dimension of Weber's thought, failing to register, for example, the irony and ridicule that complicated his admiration for Puritan asceticism. Nonetheless his analysis of techniques of self-empowerment makes for a challenging reading of familiar texts.

In his analysis of Mann, Goldman attempts to show how the writer failed to link the individual to nation or humanity. In *Reflections of an Unpolitical Man* (1918), according to Goldman, Mann conflated his personal problems with German politics; in *The Magic Mountain* (1924), the dream of a redeemed humanity supposedly never took on reality for the protagonist Hans Castorp; in *Dr. Faustus* (1947), Goldman finds a Christian humanism unacceptable for the late twentieth century. While often insightful, the analysis of

Mann is incomplete. Important political stories like "Mario and the Magician" (1930) and "Disorder and Early Sorrow" (1922) receive no mention even though they could have tightened the book's rather distant parallels between Mann and Weber. Nor does Goldman account for Mann's use of comic techniques to deflate the German educated elite's pretensions to grandeur.

Goldman's conclusion calls for more "unmasking" of the cultural ideals of Weber and Mann through psychological and social analysis (p. 273). This is a valid form of critique that Weber and Mann already turned on themselves, their class, and their country with remarkable honesty. Goldman's return to their project of unmasking suggests how faithful his book remains, despite its treatment of them as representatives of a bygone bourgeois era, to these two masters of ideological critique.

HARRY LIEBERSOHN
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DAN S. WHITE. *Lost Comrades: Socialists of the Front Generation 1918–1945*. Cambridge: Harvard University Press. 1992. Pp. viii, 255. \$34.95.

Dan S. White examines an interesting cohort of gifted European socialists who shared the wrenching experience of World War I—whence the designation "front generation" in the book's subtitle—in order to demonstrate the war's indelible impact on their lives and careers. Their writing and speaking talents were initially expended in fervent efforts in the 1920s to shake the inertia and outmoded ideas of the pre-1914 generation of European socialist leaders. Failure, frustration, and impatience led several of them on a strange pilgrimage to fascism, collaboration, and betrayal. The book focuses principally on the Flemish-born Hendrik (Henri) de Man in Belgium, whose much discussed volume in 1926 exhorted socialists to go "beyond Marxism"; on Marcel Déat in France; and on Sir Oswald Mosley in Britain. Additionally, it examines lesser-known yet equally able German socialists who also fought an unsuccessful campaign for their party's revitalization but who later under the Third Reich entered the Resistance and fell victim to the Nazis.

In the second phase of their careers, the prolonged Depression and Hitler's triumph in Germany evoked more extreme responses from the socialist rebels. The Depression led them to the blandishments of economic planning with clearly corporatist overtones. When Mosley as a junior cabinet minister in 1929 failed to persuade the Labour government to adopt his ideas on planning, he resigned from the government and the party. By 1934, he had founded the British Union of Fascists, replete with a black-shirted militia. Déat, along with his "neosocialist" associates, pressed so vigorously in 1933 for an authoritarian

program of action—with “order, authority, nation” as the rallying cry—that Léon Blum described himself as “frightened” (*épouvanté*) by the words he was hearing. France’s military defeat in 1940 afforded Déat an opportunity to become a leading collaborationist and to achieve some of the power and influence earlier denied him. By 1943 he was mouthing Nazi phrases about “Jews as parasites.” De Man achieved distinction in his own country before the war, headed the Belgian Socialist Party, and drew up the *Plan du Travail* that was adopted by his party in 1933, but he was thoroughly disgruntled at the government’s failure to put the *Plan* into action. He, too, accepted Hitler’s “New Order” after June 1940 and was soon describing National Socialism as the “socialism of the future.”

Oddly enough, these men all achieved a measure of rehabilitation in later years. White’s purpose in this critical study is not to add to the adulation, although he believes that they each merit recognition for their efforts to overcome the limitations of interwar socialism. One has to admire the deft way in which he juggles the lives and careers of his protagonists in parallel and overlapping accounts. Only occasionally does the focus become blurred, as when he informs us: “Pilgrims are not necessarily converts” (p. 8). But pilgrims, too, can lose their way. Singularly omitted also is the cohort of “front generation” socialists who in 1920 rallied to the new and then vibrant Communist Party, another kind of pilgrimage. But we are indebted for a perspicacious in-depth examination of the lives and careers of this selected group of one-time socialists, lost comrades indeed.

JOEL COLTON
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MAGDA ÁDÁM. *The Little Entente and Europe (1920–1929)*. Budapest: Akadémiai Kiadó. 1993. Pp. 329. \$40.00.

When, in 1921, Romania joined Czechoslovakia and Yugoslavia in a diplomatic arrangement aimed at combating Hungarian treaty revisionism and a restoration of the Habsburg dynasty in Budapest or Vienna, a Hungarian journalist mockingly called the resulting pact a “little entente,” a pale imitation of the Entente Cordiale. The name caught on, and the Little Entente soon became a fixture of European foreign relations. For more than a decade the alliance served its purpose: Hungary was kept in its weakened position and no changes were made in the territorial status quo in Eastern Europe. The revival of Germany in the mid-1930s, however, soon revealed the inherent flaws and weaknesses of the alliance. It was easy for the three countries to agree to thwart Hungary, but they were unable to forge a common policy toward Nazi Germany. The destruction of Czechoslovakia in 1938–39 was thus quickly followed by the official demise of the Little Entente, although it had

ceased several years earlier to play an important role in diplomatic affairs.

Few historians of Eastern Europe have been successful in writing about the Little Entente, not only because the topic requires command of a plethora of languages but also because it has continued over time to bristle with nationalist controversies. There is a certain irony to the fact that the most objective book to date should be by a Hungarian historian, Magda Ádám. She has conducted a painstaking study of an enormous amount of archival records in order to explore the diplomatic relations of the countries of the Little Entente with Hungary and the great powers in the 1920s. Because Ádám is the first historian to make a systematic investigation of recently opened French diplomatic records, she is able to throw new light on such questions as the formation of the Little Entente, the restoration attempts of Charles IV, the abortive Czechoslovak-Hungarian negotiations of 1921, and the impact of the Locarno Pact on East Central Europe.

The book is particularly valuable for the insights it provides into the ambiguous nature of French policy in the early 1920s. Ádám provides convincing documentation for her thesis that France was initially unsympathetic to the formation of the Little Entente. She traces the bewildering twists and turns of subsequent French policy and confirms that Aristide Briand secretly encouraged Charles IV to make his first restoration attempt. Even so, Ádám’s explication does not supplant the earlier work of Piotr Wandycz (*France and Her Eastern Allies, 1919–1925* [1962]), who did not have access to all the French records but nonetheless provided a more comprehensive and nuanced interpretation of French policy making. It must also be noted that because Ádám was unable to make use of materials in the Public Record Office in London, her treatment of British policy is less authoritative. Moreover, her bibliography does not list several important studies of British policy, including Marie-Luise Becker’s *England und der Donauraum, 1919–1929* (1976).

Ádám’s work is most valuable for the important new information she has discovered in the French and, to a lesser extent, Italian, Czechoslovak, and Hungarian archives. She treats her subject in a thorough and even-handed manner, free of the ethnic and nationalist biases that colored most previous historical studies of the Little Entente. Specialists will find much of interest in the book, and the general reader will learn a good deal about the intricacies and occasional absurdities of international relations in Eastern Europe. The book is not easy reading, however, and not simply because of the occasional awkwardness of the English translation. For the most part this is traditional diplomatic history, detailed and reliable but at times tedious and colorless.

THOMAS SAKMYSTER
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DAVID LOADES. *The Tudor Navy: An Administrative, Political and Military History*. (Studies in Naval History.) Brookfield, Vt.: Scolar Press. 1992. Pp. 317. \$69.95.

All students of Tudor government and English seafaring will need to consult this important book by David Loades. As the first general history of the Tudor navy in nearly a hundred years, it is sure to become the basis of future textbook accounts, as it builds on, and in innumerable places supersedes, the work of Michael Oppenheim (1896), W. Laird Clowes (1897), and J. S. Corbett (1898–99). Loades organizes his story chronologically for the whole of the Tudor era, systematically surveying policy, finance, administration, and military action during each reign. In this way he shows that the development of the navy was not sporadic, as those preoccupied with dramatic events under Henry VIII and Elizabeth I have thought, but connected and continuous. Moreover, because at every point Loades has undertaken original archival research—he cites manuscripts in five British repositories as well as the Archivo General de Simancas in Spain—he is able to say something new about the way each of the Tudors employed armed vessels. Methodologically, this approach enables him to argue convincingly that Tudor maritime history cannot be written or understood apart from the history of the Tudor state: the navy was inseparably an aspect of Tudor government.

This observation underscores the fundamental difference between the medieval navy and that of the first two Tudors. Although the ships of both Henry V and Henry VIII, for example, advertised the personal power of self-styled “imperial” kings, Henry V’s navy consisted for the most part of private, commercial vessels impressed into military service in time of war and used typically for the transport of troops. By contrast, Henry VIII’s great carracks formed the core of a new standing navy; his were cannon-carrying ships built exclusively for battle but no particular war. The origins of the new-style navy can be traced to the launching of Henry VII’s earliest “Great Ships” in 1488, the unusually large, extensively armed *Regent*, which displaced 600 tons, and the *Sovereign* of 450 tons. Although both lacked true gun decks, their castles bore portholes designed specifically for serpentes, or seagoing artillery pieces. Both were forerunners of three revolutionary vessels built by Henry VIII in 1509–13, the thousand-ton *Mary Rose* (the first to employ lidded gun-ports, an innovation of Henry VII’s shipwrights), the *Peter Pomegranate*, of about the same tonnage, and the *Henry Grace à Dieu*, the king’s gigantic flagship, which at 1,500 tons “was the most powerful warship the world had ever seen” (p. 67).

The material requirements of keeping such huge ships in a constant state of readiness spurred the development of special drydocks and storehouses. In 1497, the building of such facilities, the purchasing of

stores (especially canvas and cables) and the oversight of shipwrights, caulkers, carpenters, painters, and laborers was put in the hands of a full-time professional administrator, the clerk of the king’s ships. Henry VIII’s massive shipbuilding program and the logistical requirements of his three French wars forced a formal reorganization and rationalization of naval administration in the mid-1540s. Loades argues persuasively that responsibility for the creation of the new seven-man Council for Marine Causes in 1545 lay not with Henry VIII but with John Dudley, Viscount Lisle (the future duke of Northumberland), a brilliant jousting, swordsman, and soldier who became a ferocious sea captain, master of the king’s armory, and England’s lord admiral, commanding all of Henry’s ships by 1546.

Loades has properly devoted about half of his book to administrative matters. The accounts of major engagements, including the defeat of the Armada, constitute succinct, first-rate syntheses punctuated by Loades’s judicious, often revisionist interpretations. Of necessity many numbers are cited, some of which would have been better placed in appendixes; the tonnage, arms, and known complements of selected vessels could have been listed in a separate section. It is a pity that in such a well-founded history of the royal navy there are no pictures or diagrams of ships.

DALE HOAK

College of William and Mary

ELIZABETH DARRACOTT WHEELER. *Sir John Dodderidge, Celebrated Barrister of Britain, 1555–1628*. Lewiston, N.Y.: Edwin Mellen. 1992. Pp. iv, 238. \$69.95.

Legal historians should welcome a biography of Sir John Dodderidge; that it should link his career in the law to his and familial interests in commerce and the colonization of Virginia makes it all the more intriguing. Elizabeth Darracott Wheeler offers, moreover, an escape from Whig history, for Dodderidge is here portrayed as a faithful servant of the early Stuarts. Although there remain nagging questions of his excessive subservience to them and willingness to give extra-judicial opinions, he was a learned jurist and a healthy antidote to Edward Coke. All this said, the present work is disappointing, both in substance and method.

John Dodderidge was a Devonshire man, son of Richard Dodderidge, a merchant of Barnstable. He was educated at Exeter College, Oxford, before becoming a member of the Middle Temple. As solicitor general, the king’s sergeant at law, and King’s Bench justice from 1612 until his death, he was competent (despite some lapses) and certainly scholarly. Best known of his written works, all of which were published posthumously, was *The English Lawyer*, which won praise from Holdsworth.

During his tenure as judge, Dodderidge heard many famous cases—the Postnati (1608), Commen-

dams (1616), the Five Knights (1627–38), and others—but these receive either brief comment or are ignored altogether. Why? Because Wheeler, despite the book's subtitle, appears only incidentally interested in Dodderidge as a "celebrated barrister." Rather, her focus is Virginia colonization and the roles Dodderidge and her ancestor John Darracott played in it. A clue is in her acknowledgement that she "was stimulated by my native site, the area surrounding Richmond; the home of the first permanent English settlement in America at Jamestown nearby" (p. iii).

Not only is there an absence of any analysis of cases or judgment as to their significance but also the author merely mentions (p. 190) Dodderidge's classic study, *The English Lawyer*. Wilfred R. Prest in his general work on barristers provides a sample of the kind of analysis that one expects of Wheeler (*The Rise of the Barristers: A Social History of the English Bar 1590–1640* [1991], 14–16).

I am troubled, too, by the book's structure and the author's style: the many (twenty-eight) chapters in fewer than 200 pages, which result in chapters of just a few pages; the many single-sentence paragraphs with no transition to those preceding or following; the remarkable ambiguity or naiveté of many statements, especially chapter conclusions; excessive space given to what I regard as irrelevancies; liberties that the author has taken in re-creating historical incidents and individuals' behavior and thought; anachronisms, such as Episcopal, instead of Anglican, church (pp. 106, 177); and prejudicial word choice, such as frequent referrals to Native Americans as "savages" and "natives" (pp. 129–31). Because sources for this work are exceptionally weak, Wheeler should have felt compelled to compensate by analyzing Dodderidge's writings and his cases or taking a completely different approach that would have expanded on the theme of Virginia colonization. As it is, this biography regrettably falls short in both instances.

ALBERT J. SCHMIDT
Quinnipiac College

JANE H. OHLMEYER. *Civil War and Restoration in the Three Stuart Kingdoms: The Career of Randal MacDonnell, Marquis of Antrim, 1609–1683*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1993. Pp. xxiii, 357. \$69.95.

Biography is an unfashionable, but indestructible, genre in the repertory of the late-twentieth-century historian. Jane H. Ohlmeyer's study illustrates biography's enduring utility, and its capacity—in the right hands—to make a more significant contribution than many trendier works of scholarship.

Randal MacDonnell, second earl and first marquis of Antrim, is hardly a household name, even among

seventeenth-century historians. The distinction of this study is not to promote him to heroic status but instead to use his sinuous life to clarify some of the most resistant obscurities of the three Stuart kingdoms.

Antrim, like many other seventeenth-century "survivors," changed his colors frequently. He was a wealthy Catholic of Scottish extraction with huge estates in east Ulster. His marriage in 1635 to the widow of the Duke of Buckingham dramatically enhanced his access to the Stuart court, and he became, sequentially (and at moments, simultaneously), an intimate of Charles I and Henrietta Maria, a stalwart of the Confederate Catholics of Ireland, an ally of Oliver Cromwell in the 1650s, and a prime beneficiary of the restoration of Charles II thereafter. The challenge to Ohlmeyer is to make these and other shifts of allegiance intelligible as more than caprice or mere roguery.

She succeeds, for the most part, by her learned explication of the political dynamics of the world in which Antrim lived. For example, we come to understand the marquis as neither Scottish nor Irish (in the modern sense) but rather as a man of that connection of east Ulster and southwestern Scotland that defies the watery separation of the North Channel. The very unconventionality (from an English point of view) of his "tribal" power base gave him a mobility, a suppleness, that also facilitated his dealings with what Ohlmeyer terms the "fourth and fifth kingdoms" of France, as well as the Holy Roman empire. A major player in the turmoil of the 1640s, Antrim is so much a "multiple kingdoms" figure that one might suspect Conrad Russell of having invented him, if the documentation were not so convincing.

Another illumination comes from Ohlmeyer's investigation of Irish privateering, mainly from Wexford, during the 1640s. The scale of operations was staggering, and Antrim's benefits, as one of the principal organizers, helped to keep him afloat when the Irish wars deprived him of revenue from his Ulster estates. Thus, the attempt to make sense of Antrim results in making greater sense of the world in which he lived.

Where Ohlmeyer is perhaps less successful is in claiming a certain luster for Antrim as a "survivor." To be sure, survival is no mean feat, but a final chapter that flirts with glorification and a somewhat strained definition of "patriotism" rings hollow in comparison with the substantial revelations that preceded it. Perhaps this brings us to a lingering paradox about biography. The biographer is expected, perhaps unfairly, to alter the way we view the subject: either to promote the person to importance or to cast that person from some undeserved pedestal. But Antrim emerges in this study not so much transformed as explained, while our understanding of the complex interaction of the Stuart kingdoms—in both

their Atlantic and European contexts—is materially changed and dramatically advanced.

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KEVIN SHARPE. *The Personal Rule of Charles I*. New Haven: Yale University Press. 1993. Pp. xxiii, 983. \$45.00.

Were there eleven years of tyranny? Did what happened during the 1630s play a decisive role in bringing about the conflicts and civil wars of the 1640s? Kevin Sharpe's 950-page examination of King Charles I's personal rule argues that the king who was executed as a traitor in 1649 was neither a tyrant nor an absolutist and that his personal rule did not evoke the opposition that fought against him a few years later. This is a comprehensive piece of scholarship that integrates Sharpe's own extensive research into both public and private archives with the work of other scholars. Sharpe looks at personalities and institutions, court and country, foreign and domestic affairs, and art and literature. The book's scope alone demands our attention. Although its length makes it burdensome to hold or carry, I found it difficult to identify material that should have been cut and from time to time even wished for more coverage of particular topics. The book has been beautifully produced, with illustrations that are not merely decorative but an integral part of Sharpe's argument. The notes assist readers in identifying the sources that are such an important part of the book's contribution to scholarship. The absence of a bibliography is regrettable; its inclusion would not have added much to the size or cost of an already large volume.

Organized chronologically into six major parts, each including topical chapters with further subdivisions, the volume begins with the circumstances that led to the dissolution of Parliament in 1629 and concludes with the king's decision to call Parliament in the autumn of 1640. Sharpe focuses on 1637 as the time when the peace that had previously characterized the era of personal rule gave way to war against the Scots. He agrees with other scholars that the war between the kingdoms was of critical importance in bringing the personal rule to an end, but the interpretation of the war is his own. Sharpe's overall scheme works well, but within chapters the topical arrangement produces some seemingly abrupt transitions.

This volume deserves careful reading even from those scholars whose familiarity with Sharpe's previous work means that they will not be surprised at the overall conclusions he reaches here. Those who undertake studies of the 1630s in the future will have to take into account the qualitative and quantitative evidence that Sharpe has produced and his carefully argued challenges to previous scholarship. His assess-

ments of the Caroline court and Privy Council, the collection of shipmoney, or the state of the church, for example, cannot be ignored.

Charles I is the key figure in the story that Sharpe tells. The picture of the king that emerges amplifies and revises that of previous works, but it remains unsatisfactory. As a man who appreciated images rather than words and committed himself to upholding tradition and ruling his realm in accord with his principles, Charles appears as a victim: of circumstance; of institutions that were unable to meet the challenges of the times; and of the Scots whose propaganda and friends worked against him in England. If we concede that the king faced enormous problems, must we accept his earnestness without denying such shortcomings as the failure to perceive that the consequences of upholding principle might lead the kingdom into turmoil? Were royal policies more important than royal style?

Scholars may want to take issue with Sharpe on the questions he asks, his conceptual framework, and his interpretation of the evidence in many instances. For Sharpe, power is concrete. In discussing the court and the Privy Council, for example, he seems to measure the importance of individuals by the papers they handled, meetings they attended, and offices they held rather than by force of personality. However difficult, an assessment of power needs to include intangible influence, and consideration of discontent during the 1630s requires grappling with those who were grumbling and dissatisfied but ostensibly obedient.

An underlying theme of this book, like many about early seventeenth-century England, is how to explain the outbreak of war in 1642. Here, too, the way Sharpe poses the question is critical. In rejecting religion or finance as major factors in the choice of sides in the civil war, he neglects to see that these factors may nevertheless have been genuine grievances; they may have led individuals to the point where they could participate in the conflict although a host of other considerations, often very personal in nature, entered into their determination of allegiance once war broke out.

In dealing with questions of religious and constitutional conflict, Sharpe emphasizes theory and doctrine. These are the stuff of learned and legalistic controversy, but when they enter politics they are often modified. Labels are applied loosely and perceptions may become more important than doctrinal purity. This was certainly true with regard to arminianism: in addition to knowing about it, we need to know about perceptions of it. Because Sharpe dismisses the polemics against William Laud and the bishops as the work of extremists, he avoids attempting to explain the power those polemics assumed. One of the most unsatisfactory aspects of the book is the treatment of Puritanism. Without offering a clear definition, Sharpe calls Puritans disloyal and suggests that "it is far from clear that the conflict and align-

ments of 1642 owed much to the religious disagreements of the 1630s" (p. 732).

The interpretive scheme, however controversial, is one of the strengths of the book. Readers are treated to analysis; they are not left to wade through a mass of facts. Sharpe provokes us to reconsider the era of Charles I's personal rule. History will benefit if those of us who are unhappy with the questions he asks and the answers he reaches undertake more research.

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PEREZ ZAGORIN. *Milton: Aristocrat and Rebel*. Rochester, N.Y.: Boydell and Brewer. 1992. Pp. xii, 164. \$59.00.

This brief book focuses on John Milton's political tracts and the fundamentals of his political thought, with some attention also to what Perez Zagorin takes to be the dominant characteristics of Milton's psyche: his sense of chosenness as a poet and prophet, his fierce independence of mind and consequent primary commitment to intellectual and religious liberty, his towering ambition and self-confidence, and his sense of belonging to an intellectual and spiritual elite. These traits Zagorin thinks emerged early and remained stable throughout Milton's life, never seriously threatened by doubt, anxiety, and external crises like blindness or the collapse of the revolution.

Zagorin argues that a moral rather than a strictly political principle undergirds Milton's politics: the classical/Christian idea of an aristocracy of the virtuous to whom liberty and governance rightly belong. This moral aristocracy tends to correlate with social class, so Milton expected to find his virtuous leaders in the well-born and the "middling sort," not the masses. Zagorin sees Milton's writings from 1640 on as generally consistent: his ideal of a moral aristocracy allowed him without much strain to compromise his commitment to republicanism and popular sovereignty, given the evident failure of the English people to become the "nation of Prophets" he had hoped for in *Areopagitica* (1644). Zagorin sees Milton's poems of the 1630s as mostly apolitical, but finds *Paradise Lost* (1667), *Paradise Regained* (1671), and *Samson Agonistes* (1671) consistent with the same central political principle. They do not repudiate the revolution, although they embody a profound historical pessimism following on the defeat of the Good Old Cause.

This is hardly a new reading of Milton's life; with some shifts in emphasis it has close affinities with the "Christian humanist" Milton of the 1930s and 1940s, as constructed by James Holly Hanford, Merritt Hughes, and Douglas Bush. Nor does Zagorin claim new biographical discoveries, but admittedly draws heavily on the classic biographies of David Masson (*The Life of John Milton* [1859–94]) and W. R. Parker (*Milton* [1968]). He is aware of, and takes some

account of, recent scholarship and interpretation, but dismisses new-historicist, deconstructive, Marxist, and feminist approaches to Milton's texts, arguing against them that morality, not cultural politics, is at the center of all Milton's work. His chief quarrel—conducted rather by implication than explicit challenge—is with Christopher Hill's version of the political Milton in *Milton and the English Revolution* (1977). Zagorin's book opposes Hill's construction of Milton's poetry and prose as always profoundly political and his revisionist portrait of Milton as deriving his ideas less from reading the ancients than from dialogue and association with the political and religious radicals who were his contemporaries.

The lack of novelty is not in itself a fault; Zagorin's book offers a useful counterbalance and corrective to some recent studies, and it provides a clear and generally accurate account of the arguments of Milton's chief prose tracts. Yet it does not engage seriously enough with current scholarship to claim a place in the ongoing dialogue about the poet. Nor does it engage closely enough with Milton's texts, their order, and their contexts to make its own arguments persuasive.

Zagorin is content to rephrase central themes: he gives no attention to metaphor, rhetoric, or political and literary context. Attention to how greatly *Comus* (1634) differed from contemporary Caroline masques and how pointedly it criticized their ideology would enable Zagorin to recognize its political content. Noticing how greatly Milton's *Poems* of 1645 differed from the several collections of Cavalier lyrics published in the mid-1640s by the same publisher (Humphrey Moseley) would prompt some recasting of Zagorin's suggestion that Milton in these verses sought to distance himself from the Puritan faction and rise above the conflict. Closer attention to the chronology and language of Milton's texts would allow Zagorin to complicate his view of Milton's stable and serene self-confidence by recognizing the anxiety and ambivalence he often registered about his vocation, his marriage, his blindness, and his lofty poetic aspirations. Analysis of the rhetoric and the specific political circumstances of the prose tracts would lead Zagorin to recognize how Milton often accommodates his argument—in very political ways—to the audience he wishes to persuade, so that he sometimes proposes what he thinks possible rather than what he most wants to see. And much more than the summary sketch Zagorin offers is needed to treat the complex politics of the major poems: Satan's rebellion, God's monarchy, the Abdiel-Satan debate about the bases of sovereignty, the domestic politics of Eden, the debate about kingship and related topics in *Paradise Regained*, and especially the implications for past or future rebellions in *Samson Agonistes*.

Yet despite its limitations, this reading of Milton's life by a distinguished historian of the period is of considerable interest. This is a lucid, pleasantly written book, whose emphases produce a recognizable

line drawing of the political Milton, if not a fully realized portrait.

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W. M. SPELLMAN. *The Latitudinarians and the Church of England, 1660–1700*. Athens: University of Georgia Press. 1993. Pp. x, 228. \$40.00.

In this treatment of the Latitudinarian clergy in the Church of England during the eras of the Restoration and the Revolution of 1688, W. M. Spellman notes that, instead of being studied in their own right as a particular theological school adapting Christianity to the needs of its own age, they have misleadingly been regarded as a transition to modern rationalism. But Spellman thinks that these “moderate churchmen” were much less the precursors of Deism and the Age of Reason” and much closer to traditional Christian and Protestant thought than has usually been supposed (p. 6). Initial chapters provide an account of the rise of the Latitudinarians (emphasizing their roots in Richard Hooker and William Chillingworth) and stress their moderation in the supercharged atmosphere of the Restoration. Later chapters show that on many theological points the Latitudinarians have been misrepresented: while giving scope to reason in religion (as did many traditional theologians) they did not ignore special revelation and the atonement of Christ nor reject belief in human sinfulness and the need for the assistance of grace in order to lead a holy life.

Recent work on the Restoration church, including an earlier book by Spellman on *John Locke and the Problem of Depravity* (1988) as well as books by John Spurr (*The Restoration Church of England* [1991]) and Gerard Reedy (*The Bible and Reason* [1985]), have awakened historians to a greater complexity and richness in the Church of England in the later seventeenth century than hitherto acknowledged. Spellman reconsiders an important party in Restoration Anglicanism and lays to rest many clichés about it. Certainly Spellman has established the point that they merit study in their own right, being neither negligible as Christian theologians nor interesting merely as a way-station to something else.

Spellman also seeks to vindicate the Latitudinarians from the charge that they were proponents of “moralism,” and if by moralism one means the reduction of Christianity to a natural and self-regarding morality, he is right: they believed in the necessity of atonement and divine grace for salvation and the holy life. But “moralism” in the literature on the period has sometimes been used with a more precise meaning to refer to the rejection of certain theological teachings, accepted at the English Reformation, about justification and sanctification. For this earlier Church of England theology, grace was a transforming power rather than an assistance. However much

they accepted the fall and the atonement, the English Latitudinarians were moralists in that more precise sense of the word, by Spellman’s own evidence, as were also some of the Restoration Dissenters.

There are a few other matters with which one might take issue. For example, Spellman seems to accept at face value the accusation that antinomianism was in fact a serious threat to morality in England. He also separates the Latitudinarians from Calvinist theologians by stressing the former’s traditionally Christian harmonization of reason and revelation and their concentration on practical theology; but many Calvinists (including Calvin) acknowledged a considerable scope for human reason, and a practical emphasis in theological writings (admittedly of a very different sort) was typical of dissenting Calvinists in that era. Finally, one might wonder if, in his sympathy for the Latitudinarians, Spellman has given too little attention to their sometimes surprisingly harsh attitude to the Dissenters.

On the whole, however, this book is an important addition to the literature on the Latitudinarians, and is one among several recent books that by taking a fresh look at the sources has made a significant contribution to our understanding of the Church of England in the later seventeenth century.

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TIM HARRIS. *Politics under the Later Stuarts: Party Conflict in a Divided Society, 1660–1715*. (Studies in Modern History.) New York: Longman. 1993. Pp. xii, 260.

Tim Harris has written the finest and most comprehensive synthesis of late-Stuart politics to appear since J. H. Plumb’s *The Growth of Political Stability in England 1675–1725* (1967). Plumb wrote primarily to explain the origins of eighteenth-century politics, but his lively prose and well-crafted arguments made his study a tour de force. The “rage of party” that he traced to its Restoration origins was, however, much more a rage about power and place than about belief and ideological commitment. Different understandings of the constitution were central to Plumb’s account of the first Whigs and the first Tories, but he devoted little attention either to the ongoing confrontations between Anglicans and Dissenters or to the political language in which such confrontations were argued.

Several scholars active over the last decade, including Harris, have reconstructed the religious debates and the ideological divisions at the heart of Restoration political culture. This historiographical elevation of ideology has threatened to overshadow discussion of political structures, but in this volume Harris interweaves and balances the histories of political and religious ideas and political and religious institutions. Moreover, he successfully integrates the new histori-

ography of the Restoration with the history of the reigns of William III and Anne, as provided by such notable scholars as Geoffrey Holmes, William Speck, and Henry Horwitz. The result is a judicious and interpretive survey of the late-Stuart period as a whole that tells the story of its politics from beginning to end rather than from end to beginning.

Sweeping the field of Restoration politics clean of Whiggish interpretations, Harris cultivates the new arguments that are springing up in their places. Although he stresses that political parties did not exist before 1679 or a party system before 1689, he finds the origins of party in the unresolved tensions—especially religious tensions—that accompanied the return of the Stuarts. Taking an evolutionary approach to the development of parties, he sees organization as secondary, both in importance and in time, to common allegiances or politico-religious identities. Exploring the rich ideological interplay along both a court and country axis and an Anglican and Nonconformist axis, he explicitly awards the religious dimension priority over the constitutional. Whigs and Tories evolved by 1679 less out of the previous court and country followings in Parliament and public opinion than out of the conflict over the church. Like party rivalry itself, the convergences of low church with country and of high church with court came in response to political circumstance, were not without qualifications and ambiguities, and were drastically altered by the early eighteenth century.

Arguing for the importance of the succession issue, Harris retains the characterization of the political crisis of 1678–83 as an exclusion crisis. He demonstrates, however, that fears and arguments about the threat from “popery and arbitrary government” moved politicians, electors, and popular followings in both parties and made the future of the church settlement as much an issue as the security of Parliament or the future of the crown. Building on his previous work, Harris establishes the Tories’ ideological credentials as a party more of legalist constitutionalism than of royalist absolutism. Although he agrees with those scholars who have argued that the Whig position was essentially populist and radical, he maintains that the popularity of that position was actually short-lived.

As he moves across the conventional boundary of the Revolution of 1688–89—a boundary that has divided subsets of specialists—Harris relies less on his own reading of the sources and more on his reading of the historians. The high road of party politics after the revolution, as well as the low road of popular politics, have each been well traveled of late, but Harris is not effusive in his recognition of those who have preceded him. The postrevolutionary treatments found here of court and country, of electoral and popular politics, of church and Dissent, and of Jacobitism build on and revise previous work. Harris also places such topics in the long-term framework that has often been missing from other studies.

A highly argumentative book that incorporates, qualifies, or rejects the conclusions of so many scholars working throughout an entire field is bound to elicit spirited questions and responses. Were the religious and philosophical grounds on which many dissenters accepted royal indulgence in 1672 and 1687 so close to court principles? If party is defined primarily in terms of allegiance, was the crushing of the Whigs after 1681 so complete and devastating? If the Glorious Revolution is defined more as a political and religious process and less as a constitutional event, does it retain its significance? And how are we to understand the history of public opinion in this period? Harris’s study will provoke and sustain scholarly inquiry into late-Stuart politics for some time to come.

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BEVERLY LEMIRE. *Fashion's Favourite: The Cotton Trade and the Consumer in Britain, 1660–1800*. (Pasold Studies in Textile History, number 9.) New York: Oxford University Press. 1991. Pp. viii, 244. \$65.00.

Beverly Lemire’s welcome and mainly successful exploration of the home market for cotton textiles presupposes the reader’s acquaintance with the great mechanical innovations in textile manufacture. Scant attention is paid to the early English cotton industry, and the first chapter, based largely on secondary works, deals with the wide use of cloths from India. The later prohibition of many of these imports is well treated save for a lack of due appreciation of the effects of the general economic recession that was marked by the South Sea Bubble.

Later chapters deal fully with the domestic market for cotton textiles in the eighteenth century; the wholesale, retail, and second-hand trades; cloth-finishing; household cloths; and cotton clothing, with special attention to the changing fashions in feminine attire among all ranks of society. In these chapters Lemire makes good use of many previously neglected primary sources, one of the most interesting being John Holker’s collection of swatches in Paris. The black-and-white illustrations are aptly chosen. Four appendixes demonstrate the wide range of Lancashire textiles in the period 1775–85; the ownership of sundry domestic items, including many cotton pieces, between 1732 and 1793; the journeys of a typical commercial traveler in the years 1734–37; and the extensive network of customers served by one Manchester firm. Unfortunately, neither a bibliography nor a list of manuscript sources is included.

This may well prove a gripping book, but readers must remain on their guard, to which they will be alerted by such spelling errors as “dowlass,” “hessen” (p. 107), and “callamanco” (p. 138). Elsewhere, too, there seems to be a total lack of awareness that many fabrics, such as tufts, everlasting, swansdown, and

tammy, could be woven in different fibers, of which cotton wool was only one (pp. 78, 101, 105). This lack is especially marked in passages where all cottons are assumed to have incorporated cotton wool, whereas woollens that had been finished by the cottoning process, and therefore called cottons, were still on sale in the eighteenth century (pp. 90, 103, 181, 184).

Comparisons between the book and its manuscript sources are also disquieting. For instance, the chapman employed in the southwest was named Lupton, not Lomax (p. 119). The quotations are not all wholly accurate. "Ready" is misread as "rady," "Easterly" as "Costerly" (p. 123), and "now" as "how" (p. 124) while "are" is omitted before "arriv'd" and "enquire at" before "Mr Kellands" (p. 124). Some readers may look askance at such sentences as "The gentry and middling ranks bought clothes for reasons other than necessity, and when no longer needed these clothes found their way back on to the market" (p. 67), or "However, prohibition and reordered the domestic market" (p. 78). Nevertheless, despite its many minor blemishes, this remains an interesting and worthwhile book.

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PHYLLIS MACK. *Visionary Women: Ecstatic Prophecy in Seventeenth-Century England*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xiii, 465. \$40.00.

Phyllis Mack introduces a fascinating cast of characters in this study of English visionary women from the civil war through the seventeenth century. Mack is interested in ecstatic prophecy, the distinguishing activity of the visionary, and focuses on the Quakers, the community in which female visionaries were most numerous and most respected. Quaker radicalism initially conflated masculine and feminine attributes, later used traditional gender roles to enhance women's formal authority, but in both instances allowed women to function legitimately as prophets, preachers, missionaries, and "mothers in Israel." Mack establishes a relationship between ecstatic prophecy and the culture of gender. One need not embrace all her conclusions to recognize that she has written a valuable book addressing the interaction of gender and spirituality in early modern England.

Nearly three hundred visionary women were active during the civil war and Interregnum, more than two-thirds of whom were Quakers. Using gendered language, sometimes speaking emotionally or from a trance, the female visionary created confusion and fear among an audience forced to decide whether they were listening to a prophet, a witch, or merely a hysterical female. The non-Quaker visionary claimed to be a frail and empty vessel, a passive instrument of divine revelation. The Quaker claimed within her

own soul "the light" or God's voice, whose truth she preached during moments of ecstatic prophecy. Theology thus led Quakers to see visionary women (and men) as heroic figures, biblical personae, whose gender and class were irrelevant.

Rich in analysis and example, the book opens with a discussion of "images and stereotypes about women that pervaded the culture" (p. 18) and emphasizes "the fluidity of the feminine archetype" (p. 24). So mutable was the archetype and so great were the fears surrounding the magical and occult powers of women that female visionaries even described male enemies with negative feminine imagery. Quakers adopted traditional gendered language yet saw themselves "as souls with the potential for both masculine and feminine expression" (p. 237).

A great strength in the book is Mack's willingness to address complexities and contradictions within her sources. She notes "the inadequacy of words like 'advance' or 'decline' for historians of gender" (p. 412), a point demonstrated through her analysis of Quakerism after 1660. The movement grew structured and bureaucratic, as Quakers, responding to threats of antinomianism and persecution, accepted the discipline of meetings and more conventional gender roles. Mack rejects a simplistic interpretation of these developments. She argues that "mothers in Israel," newly empowered, served conservative and progressive purposes alike. Women used traditional gender identity and separate meetings to exercise novel authority over men, authority based on women's own worth and "no less innovative because limited to matters of hearth and home" (p. 292).

Mack leaves some issues unresolved. Her discussion of Quaker marriage emphasizes spiritual egalitarianism beyond Puritan tendencies (pp. 226, 258), but elsewhere she states that "Quakers shared prevalent assumptions about the subjection of women in marriage" (p. 177). Mack makes valid distinctions between the Quaker visionary and "Puritan goodwife," but her analysis of the Quaker concept of community and sanctified life overlooks Puritan precedents that affected gender relations.

Most earlier historians discussed the work of female visionaries "from every perspective *except* that of religion" (p. 87). To redress historiographic balance, however, Mack introduces a problematic dichotomy between religiosity and the believer's sense of self. She argues that because female visionaries sought self-transcendence, their behavior was not self-expressive or self-assertive. This thesis is introduced early but is developed for Quakers, whose goal "was to suffocate impulses toward personal expression and achieve the annihilation of the thinking self" (p. 142).

Although Mack sensibly argues that the ideal of self-transcendence differed profoundly from our concept of self-realization, her reluctance to recognize self-expression and assertion in the behavior of visionaries remains troubling. Spirituality and religious ecstasy enhanced rather than denied the power

of the individual. Attempts at self-transcendence were themselves acts of will and self. Mack writes that "women as prophets were [not] devoid of personal ambition . . . but had a different, more complex view of the self and of the meaning of personal success" (p. 5), yet her subsequent analysis fails to develop along these lines.

In the end, the coherence of the argument falls apart, largely under the weight of its own qualifications. Visionaries were not only prophets but also itinerant ministers, social critics, and mothers in Israel, roles that required a strong sense of self. Among the Quakers, visionaries from the south "frequently placed their own individual struggles at the center of their published spiritual narratives" (p. 189). Mack concedes that men, unlike women, experienced some sense of personal identity and "expressiveness." Her explanation for the distinction: women, burdened with responsibilities of marriage and motherhood, "experienced their visionary moments as personal catharsis, [but] . . . acknowledged the experience purely as a catharsis of sacrifice" (p. 185). Rhetoric about sacrifice, however, may have expressed conflicting personal agendas as easily as denial of self. This book therefore will stimulate and influence further discussion about the meaning and implications of female spirituality.

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BILLIE MELMAN. *Women's Orients: English Women and the Middle East, 1718–1918; Sexuality, Religion and Work*. Ann Arbor: University of Michigan Press. 1992. Pp. xix, 417. \$39.50

If Edward Said's original work on "Orientalism" can be faulted for neglecting gender as a factor of analysis, Billie Melman seeks to rectify this omission with an examination of "the ways in which Western women experienced the Middle East, in the contacts with Middle Eastern women, in the limits of their vision of the 'elsewhere' and representations of it" (p. 1). Melman asks whether educated English women, themselves conventionally identified as the "other" within their own dominant culture, developed a gender notion of the "otherness" of those outside that culture? She is interested in whether gender and class differences mattered more than national, religious, and racial differences, and she wonders if women developed a separate "feminine" experience in the expansion of empire. She begins with the anonymous publication of "letters" about Turkey by Lady Mary Worley Montagu, for they "set the terms of an alternative, gender-specific discourse on the Middle East . . . which evolved alongside the dominant discussion, which nowadays is described as Orientalism" (p. 2).

Understanding "Orientalism" as signifying "a representation of the other that is based on a hierarchical

relationship between an hegemonic and a subordinate group" (p. 4), Melman notes that such representations are interpreted as involving the feminization and eroticization of the "changeless" East. This metaphor makes the Western voice a male one, synonymous with cultural and political authority. She argues that "an alternative view" developed alongside the dominant one. Drawing on 245 printed works of 187 women printed between 1821 and 1914, and a hundred more uncatalogued writings that she has found, she concludes there existed a plurality of discourses. These encounters with polygamy, concubinage, and sequestered women resulted in self-critical analogies with the monogamous West, substituting a solidarity of gender for sexual and racial superiority. The result was not cultural smugness but sometimes "an identification with the other that cut across the barriers of religion, culture and ethnicity" (p. 8).

This alternative discourse was possible because it evolved outside the formal metropolitan sites of male knowledge and power, such as learned and professional societies. Women established their own agency through voluntary, nonspecialist societies interested in the Middle East and in evangelical proselytizing organizations. One might question Melman's assumption that if these groups were less directly connected with imperialist policies, they were not implicated in imperialist expansion; but she does establish her point that women's experience of the Middle East was more individual and private than institutionalized and public. She also notes that just as the rise of scientific interest in the area was related to European power and politics and the decline of the Ottoman empire, so the search by Ottoman elites for military and technological answers promised by modern industrialization facilitated travel under consular protection for European women and men, whether on Cook's tours or institutionally connected trips.

Travel to the Middle East became a middle-class Victorian passion, while John Bunyan's *Pilgrim's Progress* remained a powerful metaphor in the half century down to 1914. During this period, Western women outnumbered their male counterparts as missionaries and in religious and educational work directed to women and children in the Middle East. The harem literature and the travelogues that resulted, despite their authors' commitment to European cultural supremacy and empire, exhibited a level of sympathy with women's condition that resisted the essentialism of the common male Orientalist image of the sensuous and libidinous *orientale*. Melman suggests the subversiveness of these accounts, finding them either a "critique on Western patriarchy and the position of woman in the West" or, by the very experience of travel, a challenge to the Victorian ethos of domesticity.

With examples of the shifts in the sensibilities of English women traveling in the Middle East since the eighteenth century—with important sections on Harriet Martineau and Amelia Edwards in the nineteenth

century and writing couples such as Isabel Burton and Richard Burton and Anne Scawen Blunt and Wilfrid Scawen Blunt—Melman demonstrates the many facets of these gendered voices and reveals women in active roles. An appendix provides brief biographies of seventy-eight women travelers and travel writers, while the book is illustrated with a fine collection of prints and photographs of Christian, Jewish, and Muslim women of the Middle East. The author's citations and bibliography reveal a wealth of sources. Unfortunately, a great number of typographical and some factual errors—of names, dates, and biographical information—mar the text, as does an erratic use of the italic in the endnotes.

Despite the fairly numerous errors, this study is a welcome addition to a growing literature on European women travel writers who both participated in Western imperialism and expressed its formulation in different and gender-specific ways. The book raises provocative issues and suggests complexities that deepen our understanding of these colonial exchanges and representations.

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JEREMY BLACK. *Pitt the Elder*. (British Lives.) New York: Cambridge University Press. 1992. Pp. xviii, 320. Cloth \$49.95, paper \$14.95.

Jeremy Black has written one of the first volumes in a potentially exciting new series, "British Lives," edited by Maurice Cowling and John Vincent, that will present biographical studies of major British or British imperial figures "whose current interpretation has become stale or conventional." Although I am not sure that the "stale and conventional" criterion applies to recent studies of the elder William Pitt, Black jumps in headfirst to reclaim an older explanation for Pitt's career, one which may not be unsympathetic to the historical tastes of Cowling and Vincent.

Most recent accounts of Pitt's ministerial career, and especially of the great Newcastle-Pitt ministry of 1757–61, have tended, as in the work of Marie Peters (*Pitt and Popularity* [1980]), Stephen Baxter (*England's Rise to Greatness, 1660–1763* [1983]), and, most notably, Richard Middleton (*The Bells of Victory* [1985]), to qualify the traditional view of Pitt as a supreme war leader and strategic genius who was directly responsible for the astounding British victories in India, Canada, and the West Indies. Was Pitt any more responsible for the victory on the Plains of Abraham than Lord Liverpool (or the Prince Regent, who thought he had personally won it) for the Battle of Waterloo? Black is sensitive to revisionist concerns and always properly conscious of the difficulties of close London supervision of early modern imperial wars. Yet in the end Black is a Carlylean. His Pitt is a "Hanoverian hero," a "hero for a country that gave

that description to no other politician" (p. 309). Mischievously, Black can barely contain his scorn for the failure of "modern academics" to laud "men of action" and "politicians" while reserving their plaudits for "intellectuals and bureaucrats . . . women, peasants, criminals, and rioters" (p. 308).

This volume is largely based on unpublished manuscript sources, reflecting Black's usual *modus operandi*. Eighteenth-century scholars will be interested in the extensive use of diplomatic sources from the archives of Turin, Paris, Hanover, and Munich. These reveal a European view both of Pitt's career and the nature and organization of British politics, a perspective often missing from British texts.

Black is particularly discerning on Pitt's private life, and that life was largely circumscribed by illness. It is difficult from this distance, given the state of medical knowledge in the eighteenth century, to pinpoint exactly the sources of Pitt's lifelong afflictions. Black considers the possibilities of hereditary madness, bowel problems, venereal disease, psychosomatic disorder, clinical manic depression, and gout. Whatever ailed him, Pitt sought solace in prodigious drinking.

It is clear that in many ways the younger William Pitt, in his avoidance of marriage and in his alcoholism, was a chip off the old block. Indeed, the father finally married at the same advanced age as the bachelor son died. There are other resemblances between the two men. Black shows that in 1761, flushed with his victories, Pitt was hailed in toast and song as "the National Pilot" (p. 203). One wonders if George Canning, who in 1802 wrote the most famous political song in English history for the younger Pitt, "The Pilot that Weather'd the Storm," realized this parallel.

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PAUL DAVID NELSON. *General James Grant: Scottish Soldier and Royal Governor of East Florida*. Gainesville: University Press of Florida. 1993. Pp. xi, 207. \$29.95.

This is a traditional biography of James Grant, a Scottish soldier, royal governor, member of Parliament, and laird of Ballindalloch Castle in Scotland. Through extensive research, Paul David Nelson brings new material to previous studies by use of part of the Grant papers in the private library of the Macpherson Grant family at Ballindalloch Castle.

James Grant had a long and at times controversial career in government service. A native of Scotland, he demonstrated strong loyalty to Clan Grant and later to the Hanoverian monarchy of Great Britain. His military duty began with the Royal Scots regiment with service on the Continent of Europe in the War of Austrian Succession. Coming to America in 1757, he joined the expedition of General John Forbes and Colonel Henry Bouquet against Fort Duquesne the

following year. Although ingratiating himself with both commanders, he carelessly fell captive to the French but was later released. Recovering from this humiliation, he returned to military service under Colonel Archibald Montgomery against the Cherokee Indians in 1760, and he commanded a second expedition the following year that brought peace in the Cherokee War. Conflict with Colonel Thomas Middleton of the South Carolina Provincial regiment led to a bloodless duel between the two. Grant ridiculed the colonials by such statements as "the Rangers are good for nothing" and the officers were worse "if possible" (p. 33), a view that was later reversed with the success of the rebels in the American Revolution.

Grant became royal governor of East Florida in 1763. He shared in the large land grants with a profitable indigo plantation and reveled in social events by entertaining friends with food and drink, one of his stated ambitions in life. He returned to Scotland and as the second son became heir to Ballinalloch Castle on the death of his older brother. He won election to Parliament in 1773 and served intermittently to 1802.

Grant had returned to America to serve as a military officer in the revolution. He was successful in establishing great rapport with the British officers William Howe, Henry Clinton, and Charles Cornwallis. Advocating a "hard line" against the American rebels, he had some success in battles such as Brandywine but created a controversy by not following Clinton's orders at the battle of Monmouth Courthouse. Other questions arose over his command of the expedition to the West Indies, where he successfully seized St. Lucia from the French but failed to occupy Grenada and St. Vincent. He continued to be a staunch supporter of Lord North during the revolution and later of William Pitt the Younger in Parliament. He remained on active duty in the military during the challenges of the French Revolution before retiring in 1796 after fifty-five years of service.

This study follows Grant chronologically through his varied activities. Nelson gives him the benefit of the doubt about the controversies in his career and is inclined to dismiss too lightly the contrary voice of his critics. Grant was apparently a better strategist in military planning than in execution of tactical maneuvers, but one learns little about the complexities of these plans. There is even less about the identification of the Indians against whom Grant campaigned in the Cherokee War and the Creeks with whom he negotiated as governor of East Florida. Nonetheless, this biography provides the most complete account available for this colorful if arrogant figure.

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JAMES RAVEN. *Judging New Wealth: Popular Publishing and Responses to Commerce in England, 1750–1800*. New

York: Clarendon Press of Oxford University Press. 1992. Pp. viii, 327. \$60.00.

There was great irony in the judging of new wealth in late-eighteenth-century England: it judged itself. As commerce, trade, and business expanded, consumers in the marketplace of public opinion, themselves an outcome of British commercial expansion, determined what the response to commerce would be. "New wealth—in the form of middle-class readership—sought legitimacy" (p. 257) and constructed it with the purchase of popular literature that reflected and shaped its own values.

James Raven's intricate argument about how new wealth and the newly wealthy were received by English society begins with the valid assumption that the evidence for his case rests in the massive literary output of the period, in the "literary reception of the commercial revolution" (p. 157). His argument then moves to its two critical components. First, the structural: he reconstructs England's literary market in the second half of the century—its writers, printers, booksellers, critics and reviewers, readers, literacy rates, literary production, prices, and wages—all the people and relations in the matrix that constituted the commercial world of print. Second, the content: he provides an elegant analysis of what the literary output actually said about his central theme.

The lineage of Raven's research on the literary market and the world of print may be traced to Marshal McLuhan, by way of Alvin Kernan, Elizabeth Eisenstein, and Walter Ong. To some extent, the medium (of print) is the message. But Raven moves beyond what he calls "technological determinist" approaches to intellectual history to an analysis of "the commercialization of the book trades" (p. 47), or the market forces that shape the history of ideas.

Reading itself is a complex social act in which the consciousness of the reader encounters a text that has moved through the market from production to consumption as a commodity. The logic of the literary market was such that the new wealth of the century generated the readership that was the market for literary texts that were the responses to new wealth. In one of the most telling assertions in the book, we learn that this was "a public anxious to read about itself" (p. 151). J. A. W. Gunn (*Beyond Liberty and Property* [1983]) made a similar kind of argument about the role of the press in the "process of self-recognition" or "social self-knowledge" (p. 5) in eighteenth-century political life. Print is not a point of presence outside of society, but part of society even while it reveals society to itself.

Although literary sources of the period display much "hostility" to tradesmen, manufacturers, and the newly wealthy, by the 1790s "social and ethical" considerations and not the raw facts of "commercial and industrial achievement" were the basis of their reaction; Raven finds no "sustained attack upon the basic capitalistic spirit" (p. 289). Popular literature

praised the "Christian gentleman merchant" but condemned "nouveaux riches traders" (p. 113). The "vulgar" were distinguished from the "polite" (p. 146). Materialism, luxury, fashion, and corruption were rejected while propriety, elegance, taste, and economy were embraced. Wealth had responsibilities. In short, it was not "business activity" per se but the "misuse or under-use of wealth derived from it" that the middle class feared (p. 262). Raven brings together the two major components of his argument with the conclusion that "writers and booksellers sought for good commercial reasons to legitimize specific modes of economic and social behavior by upwardly mobile groups" (p. 13). They wrote and sold what their readers wanted to hear.

This is a kind of total history. Raven has left few sources unturned and has made good use of the *Eighteenth-Century Short Title Catalogue* for quantitative data base evidence. There is some tension in the text. The structural and content components of the argument are sometimes too dichotomized. Raven himself seems to be more comfortable with the content than the structural material. In addition to being a source of many important findings, this book is also an impressive essay on method, an extremely responsible account of what it would really take to answer some of our larger questions about this period.

ROCCO L. CAPRARO
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THEODORE KODITSCHKEK. *Class Formation and Urban Industrial Society: Bradford, 1750-1850*. New York: Cambridge University Press. 1990. Pp. xi, 611. \$69.50.

Unquestionably a major work, Theodore Koditschek's study of Bradford addresses three issues. It analyzes, first, the transformations of local pre-urban and urban organization that followed on industrialization. Second, it charts the creation of Bradford's urban-industrial bourgeoisie and the closely related rise to predominance of liberal-entrepreneurial and voluntarist ideologies. Third, it examines the ways in which early industrial elites responded to both the destitution and the episodic militancy of the woolen workers on whose labor their prosperity was based. Interwoven with these three threads are stories of what Koditschek treats, not unconventionally, as the three classes vying for power in Bradford's politics and economy: the traditional, mainly landed, elite; the mostly urban industrial bourgeoisie; and the workers.

Koditschek deserves credit for recognizing the need to pay attention to all three groups and the ways in which their activities and ideologies influenced each other. But the heart of his study is his treatment of the liberal bourgeoisie, to whose class formation the title must refer. His traditional elites appear in varying degrees of decline and disarray; his workers never quite get it together. But Koditschek offers perhaps the best local study of the processes by which

industry—in both the sense of personal hard work and that of transformed organization of production—combined with self-conscious reform of social relations and ideological commitment to form the Victorian middle class.

Koditschek approaches middle-class formation with considerable sympathy, but within a conceptual framework indebted to E. P. Thompson. This adds interest; his is no uncritical affirmation of the middle-class self-image. Koditschek uses much of Thompson's conceptual vocabulary, but disagrees about timing: Thompson's "chronology of class formation may be a bit premature; for while Bradford, by the beginning of the nineteenth century, had ceased to be a paternalist community that linked the plebeian culture with the culture of elites, it had not yet fully become a class society in which capitalist relations constituted the dominant axis on which social groups would interact" (p. 77). The implication is that Koditschek's study supports the moral of Thompson's story, but postpones its realization. This is not really so. Koditschek's narrative points to a moral more like that of Harold Perkin's account of the formation of a viable class society in *The Origins of Modern English Society* (1969). The liberals not only win but liberalism also comes more or less to work.

Traditional elites and workers play supporting roles in this drama, encouraging liberals to live up to their own more abstract egalitarianism and to fulfill more of the promises of voluntarism and civic reform. They shape the emerging consensus, but it is a liberal, middle-class consensus. "Working-class consciousness, even at its moment of greatest vitality, notwithstanding the vigor of its anticapitalist critique, was no more successful than Tory radicalism in devising viable alternatives to the emerging entrepreneurial world" (p. 564). If viability is measured by the predominant course of development, this seems largely to be true. But I do not think Thompson would have accepted such a test of viability. It would have sounded too much like "the enormous condescension of posterity."

This is not all criticism, for I think Thompson greatly overestimated the extent to which the English working class was made by the period that his brilliant narrative comes to an end, and Koditschek helps illuminate why neither the post-Napoleonic War popular radicalism on which Thompson dwelled, nor the early Chartist promise with which he closed, were able to carry the day. Above all, Koditschek shows a vitality and depth to liberal ideology—and a closeness of that ideology to actual lives—that Thompson and many others have not seen. But this makes it all the more curious that Koditschek should hint that Thompson is his major guide and close with a rhetorical gesture to Marx.

The temporal and geographic structure of Koditschek's own account offers some clues. Only in his considerations of industrialization and the transformation of Bradford community life does he look

back before the early 1820s. His emphasis is overwhelmingly on the 1830s and 1840s. His description of working-class radicalism thus focuses little on the critique of "Old Corruption" and much on Chartism. William Cobbett and Henry Hunt, T. J. Wooler and Richard Carlile do not figure. Tory radicals rate more attention, but at least in Cobbett's account of Bradford, they appear as a side-issue—perhaps because there was no local leader of John Fielden's stature. The power both of the older, more traditional sort of popular radicalism and of paternalism was greater earlier and outside the larger cities like Bradford.

Even with regard to the Victorian era, there is an uncertainty about just how to frame the worker's story. Koditschek seems sometimes to suggest that the 1840s saw a previously strong class consciousness fade: "the economic effects of prosperity . . . tended to divide skilled workers from the structurally proletarianized and to undermine that sense of commonality that had hitherto underwritten their consciousness of themselves as a single, unified class" (p. 492). Problems arose, he writes (not unlike Lenin), when workers sought to take their trade union-based class consciousness into a political arena. Later, "the crisis of 1839–40 symbolizes the acute difficulty that workers experienced in translating the traditional, particularistic values of community into vehicles for collective class organizations" (p. 500). Is the story of Chartism one in which class is incompletely forged out of more particularistic communal and craft solidarities, or one in which trade union-based class consciousness failed to make an adequate transition to political understanding and action? Or was the working class an effective force only for a few months in 1839?

In his account of elite politics in Bradford, Koditschek offers much clearer insights—and draws nicely on a wide range of local sources. He is perceptive with regard to the impact of scale on organization. He shows how traditional elites, while still in power, distanced themselves from the town in ways that would later help to marginalize them. Above all, he describes superbly the rich and vital "culture of voluntarism" through which entrepreneurial elites sought to create forms of social action and organization appropriate to their personal experience of "self-making" and their political economy. Wholeheartedly for themselves, and sometimes more grudgingly for their workers, Bradford's entrepreneurs built a range of associations, both religious and secular. By themselves, these proved inadequate to the task of urban reform and regulation of the ill-effects of industrialization, but when combined in the era of national liberal ascendancy with a willingness to reform and work through government, Koditschek shows them to be the basis of the viable class society of the second half of the nineteenth century.

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EDWARD INGRAM. *Britain's Persian Connection, 1798–1828: Prelude to the Great Game in Asia*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xx, 351. \$75.00.

The rise of Britain's Indian empire posed new challenges for the managers of Britain's foreign, military, and naval policies. The development of responses to these challenges is the subject of Edward Ingram's trilogy on the origins of the "Great Game," brought to a successful conclusion with this volume on the relationships of Britain and its Indian empire with the ramshackle Qajar state in Persia. Ingram demonstrates that the interaction between the needs of Britain's European role and the demands of its global empire required a continual series of adjustments that provoked ongoing arguments between London and Calcutta (as well as among the Foreign Office, the Board of Control, and the East India Company) about how best to balance the requirements of Indian defense, imperial prestige, and Britain's European position. This is all a far cry from the George Henty–Rudyard Kipling picture of the Great Game; if less colorful, it is certainly more illuminating about the multifaceted nature of a great power's international relations and the complex bureaucratic warfare that attends them. Ultimately the balancing act proved impossible to sustain and, as Ingram points out, Neville Chamberlain's policies can be seen as a last desperate attempt to keep all the balls in the air.

Ingram also highlights another, less noticed dimension of Indian imperial policy. The British Army had been neutered after Oliver Cromwell's Protectorate and never again posed a threat to Britain's political elite. The Indian Army was the steel framework of British rule, however, and the military played a role in Anglo-Indian society, and in the thinking of the government of India, for which there was no counterpart in Britain itself. The impact of Indian Army policies was felt far beyond its cantonments, not only because that army guaranteed British rule but also because the glacial pace of its jealously guarded seniority promotion system drove many talented and ambitious young officers to seek "secondment" to other branches of government where promotion, fame, and a return home with rank and status could be more readily attained.

With an aggressive army and a heavily militarized diplomatic service in place in India, it is not surprising that the almost manic expansionism that characterized Lord Wellesley's governor-generalship (1798–1805) found such a ready response in the Anglo-Indian world. This expansion left behind a cadre of Indian soldiers and political officials who, as Ingram demonstrates, played key roles for the next generation in the growth of British power in the subcontinent and its environs. Even after the Indian empire was stabilized, it would remain suffused by its army's outlook. It may well be that only the "Indianization" of that army forced by expansion during World War

II averted the sort of die-hard defense of empire that the similarly structured French colonial army launched in Algeria.

Written with great vigor and lightened by nice touches of wit, Ingram's work deserves an audience beyond those specialists for whom it will be required reading, if only because the problems of acting the role of a global power are not of merely historical interest.

RAYMOND CALLAHAN
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J. F. COAKLEY. *The Church of the East and the Church of England: A History of the Archbishop of Canterbury's Assyrian Mission*. New York: Clarendon Press of Oxford University Press. 1992. Pp. x, 422. \$85.00.

The world-wide expansion of European Christianity is not only one of the great stories of modern history but also one of the most difficult to tell. The organizing story for modern religious history, the theory of secularization, places religion at the margins of history. Furthermore, the history of religious expansion is inextricable from competing tales of imperialism and anti-imperialism, in which the Christian missionary characteristically plays the role of ecclesiastical adjunct to the Western imperialist.

In this well-written history of a small but fascinating mission sponsored by the Archbishop of Canterbury, J. F. Coakley promises, and delivers, "controversy of all kinds, violence, strong-minded and eccentric characters, exotic scenes, adventures, and tragedy" (p. 1). But he attempts to engage not only a celebratory audience eager for tales of missionary heroism but also a skeptical secular audience that regards the entire enterprise as suspect.

The Church of the East was a small, largely rural remnant of the Christian church of the Persian empire, usually known as Nestorian, sometimes as Assyrian, sometimes as Syrian Christian. These Syriac-speaking Christians had flourished for a time following the Muslim conquest of Persia, but came on hard times beginning in the fourteenth century. By the early nineteenth century two surviving communities lived on either side of the border between Persia and Turkey, where they were sporadically persecuted by their powerful Kurdish neighbors. They had the further misfortune of being sought out by missionaries promoting the competing religious interests of Roman Catholicism, American Presbyterianism, Russian Orthodoxy, and Anglicanism. The competition provided short-term material gains for their clergy, but ultimately led to internal schism and entangled the Syrian Christians in the extremely dangerous politics of Western intervention in Persia and Turkey.

Leaders of the Church of England saw the Church of the East as a kindred Episcopal denomination, resisting the theological aggression of both Roman Catholics and American Presbyterians. Celibate High

Church Anglican clergyman and nuns came to live with the Syrian Christians, not to recruit them into an Anglican church but ostensibly to revive their church from within by creating schools for their children and their clergy, and by training Christian women to be fit wives and mothers. Coakley believes that this mission's distinctive rejection of proselytism removes much of the taint of Western imperialism.

But the relationship between Christianity and imperialism involves far more than proselytism. Anglican missionaries wished to persuade the Church of the East to adopt Western standards of education, publish the Syriac liturgies and creeds with modern typefaces, and pursue an aggressive program of internal moral reform. Believing as they did that the Christian church would outlast both the British empire and Western civilization, these missionaries did not understand their mission as an attempt to extend Western culture to the non-Western world. Yet in the short term the striking thing about them is their failure to transcend the limitations of late Victorian culture. It is difficult to think of a better label than "cultural imperialism" for a program of training Syrian Christian women to be fit wives by Victorian standards. The missionaries of the Assyrian Mission took seriously their own claims to be nonimperialistic, but by accepting those claims at face value Coakley has missed the opportunity to rethink the relationship between Western religion and imperialism.

JEFFREY COX
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CLARE MIDGLEY. *Women against Slavery: The British Campaigns, 1780-1870*. New York: Routledge. 1992. Pp. xii, 281. \$69.95.

In this book, Clare Midgley provides not only a much-needed narrative of the contributions made by women to British abolitionism but also a sustained analysis of the particularistic ways in which gender shaped and constrained women's involvement in opposition to the slave trade, campaigns against slavery in the British colonies, and efforts to achieve universal abolition. As considerable contributors to the literature and the finances of these movements, she argues, women channeled their influence in gender-specific directions. Thus, disillusionment with male abolitionists' tactic of parliamentary petitioning led women to initiate abstention campaigns in which individual female consumers were encouraged to boycott sugar and other slave-produced goods. Similarly, the networks of women's antislavery societies that emerged in the 1820s deployed their funds to their own abolitionist ends. In Birmingham, this policy of selective funding sustained a cadre of traveling antislavery agents; in other regions it promoted education, relief, and Christian conversion in the British West Indies. In this manner, antislavery activities came to assume a gendered configuration: male

abolitionists concentrated their efforts on the broad economic impact of slavery, whereas women emphasized issues associated with morality and individual responsibility.

But as Midgley clearly demonstrates, women abolitionists by no means confined their activities to a putatively private female sphere of influence. From the 1830s, women entered with enthusiasm the realm of parliamentary politics, joining in the mass petitioning campaigns initiated by men but changing the inflection of these campaigns as they did so. Most notably, women's preoccupation with moral issues led them to champion the immediate abolition of slavery, rather than the policy of gradualism espoused by most male abolitionists. The transatlantic focus of women's antislavery activities in the aftermath of the Emancipation Act of 1833 also served to broaden female abolitionism beyond a narrow private sphere. For although their contacts with American abolitionists led to personal friendships and familial associations, these links also enmeshed British women in the heated debates on women's political rights and the legitimacy of resistance to governments that surrounded the American abolitionists William Lloyd Garrison and Lewis Tappan.

Well researched, broadly conceived, and coherently argued, this volume adds significantly to the literature on abolitionism. Whereas David Brion Davis has emphasized the centrality of the debate over the relative virtues of free and slave labor, Midgley highlights a "contrast emphasized by women campaigners between a society run by degraded slave holders who abused black women and undermined family life and a Christian society modelled on British lines which elevated family life and women's domestic duties" (p. 103). Much work remains to be done on the connections between these two strands of abolitionist argument, on the relationship between British abolitionism and domestic agitations such as the factory reform, poor-law reform, and prison reform movements, on the attitude of working-class women to slavery, and on the impact of British missionary endeavor on abolitionism, topics about which Midgley is largely silent. This book will provide scholars who turn to these subjects with both a wealth of information and an intelligent historiographical framework on which to base their studies.

MARGOT FINN
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BARBARA CAINE. *Victorian Feminists*. New York: Oxford University Press. 1992. Pp. xii, 284. \$39.95.

With the resurgence of interest in English women's history in the 1960s, stimulated by revived feminist political movements, many scholars focused on the Victorian women's rights campaigns. By the 1970s, the scholarship had broadened and deepened, with greater concern for the texture and context of Victo-

rian women's lives, not necessarily analyzed within a framework of oppression and resistance. In recent years, historians such as Philippa Levine, Jane Rendall, Susan Kingsley Kent, and now Barbara Caine have returned to the political struggles of Victorian women with a more critical and analytic vision than that of the often hagiographic historians of the 1960s.

Caine's contribution to the study of Victorian feminism is in her analysis of the theories about gender held by women's rights advocates. She focuses on four noted Victorian activists: Emily Davies, the founder of Girton College; Frances Power Cobbe, whose work for women's rights included the right to remain single in intimate relationships with other women rather than with abusive husbands; Josephine Butler, the prime mover of the campaign to repeal the Contagious Diseases Acts; and Millicent Garrett Fawcett, the suffragist who remained active in the early twentieth century as a moderate force in opposition to the militant followers of the Pankhursts.

Caine selected these four women not only because of their prominence but also because their differing ideas about the nature and role of women illustrate the diversity and complexity of Victorian feminism. With thorough mastery of the secondary literature and extensive research in both the published writings and the manuscript collections of her subjects, Caine offers fresh insights into the core assumptions, convictions, and ambivalences of these women whose activities in the public realm are already well known. She shows, for example, that Emily Davies was much more interested in establishing the principle of higher education for women than she was in the actual students or their tuitions. Frances Power Cobbe, firm in her commitment to the moral autonomy of women, is nevertheless revealed to have been primarily concerned with the absolute duty of a daughter to her parents, which Caine describes as "a central defining feature of [Cobbe's] feminism" (p. 134). In the discussion of Josephine Butler the major theme is Butler's strong liberalism, which led her to oppose any factory legislation restricting women's work and made her see the (male-run) state that had passed the Contagious Diseases Acts as the enemy of women. Millicent Fawcett, known for her moderate suffragist tactics, became what some of her colleagues felt was immoderate in her advocacy of social purity.

Successful in her analysis of how ideas shaped action, Caine fails in her second goal of showing how biography influenced ideology. She does not succeed, in part, because of the necessary brevity of the biographical material, but also because of her deliberate decision not to use psychological insights, a limiting factor when human motivation is the subject. Ideas can have a life and power of their own, but one of the arguments of the book is that it was family background and life circumstances rather than intellectual influences that engendered feminist consciousness. The book is also weakened by the writing style, with too frequent use of such inflated words as "extreme-

ly" and "enormous," and with too many sentences structured inversely as negatives. Much space is wasted discussing questions of labels, such as whether each woman's views are liberal or conservative, when Caine herself recognizes the problematic nature of such terms. Finally, despite her emphasis on the diversity of Victorian feminism, she refers too monolithically to "the women's movement" and to "its members," as though they were card-carrying members of a unified, single organization.

These weaknesses distract from, but do not diminish, the value of this study. As a thoughtful analysis of Victorian feminist theories, this book advances our understanding of the complex ideologies of gender in nineteenth-century England. The gaps and leaps of explanation should stimulate further research into the lives and minds of these women as well as of less well-known activists in an attempt to answer the questions of motivation and influence that Caine has so suggestively raised.

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E. A. VARLEY. *The Last of the Prince Bishops: William Van Mildert and the High Church of the Early Nineteenth Century*. New York: Cambridge University Press. 1992. Pp. xv, 261. \$59.95.

Created by the Normans after 1066 for military purposes, the areas known as the palatinates possessed significant power and rights. Throughout the centuries, however, the crown worked to curtail their privileges. A bishop ruled the palatinate of Durham, exercising certain jurisdictional rights until 1836. Durham, therefore, might appear as an anomaly in the ecclesiastical and political structures of an industrialized nineteenth-century England. The last Prince Bishop of Durham, William Van Mildert (1765–1836), did not enjoy the vast powers of his medieval predecessors, but he attempted to use his position to influence the political and religious climate of England in the turbulent 1830s. E. A. Varley's significant study skillfully weaves together Van Mildert's life and his relationship to high-church Anglicanism, especially the Hackney Phalanx and its attempts to defend traditional church rights and privileges.

After a brief and superb historiographical introduction, Varley begins her portrait of Van Mildert by describing the theological world of the late eighteenth century, especially the circle of friends associated with William Stevens and how this group eventually influenced Van Mildert's thought. Proud of his Dutch origin, his interests soon developed toward education and the clerical state. After his ordination in 1789, Van Mildert held several positions, and in 1812 successfully won election to the preachship of Lincoln's Inn. This background and his knowledge of theology insured his career in the church: Regius

Professor of Divinity at Oxford (1813), Bishop of Llandaff (1819), Dean of St. Paul's (1820), and Bishop of Durham (1826). This book, however, does not merely chronicle the life of an influential churchman.

Varley gives the reader insight into the rapidly shifting religious climate during the period. Major theological developments, the nagging problems of clerical pluralism and absenteeism, the development of the Hackney Phalanx as an attempt by some churchmen to resist the zeal of reformed-minded governments, and the relationship of this high-church group to members of the Oxford movement rightly receive significant attention. Clerical life on the parish and diocesan levels can be seen through the words and actions of Van Mildert, especially his role as the founder of the University of Durham and the problems associated with administering this rich diocese. His involvement in political issues clearly demonstrates the close connection between church and state. Using Van Mildert's correspondence, Varley discusses Van Mildert's attitudes toward reform in these areas, especially his opposition to Catholic emancipation and the Reform Bill of 1832.

This book gives the reader a new appreciation of the vitality and tensions within Anglicanism at the beginning of the nineteenth century. The important chapter dealing with the origin of the Hackney Phalanx—its goals, and its real accomplishments in education, foreign missions, and evangelization—shows that segments within the church responded to the needs of a new society and recognized the need for reform of traditional ecclesiastical structures. Varley's study is essential for an appreciation of the changing ecclesiastical world of early nineteenth-century England.

RENE KOLLAR
Saint Vincent Archabbey

RYLAND WALLACE. *"Organise! Organise! Organise!" A Study of Reform Agitations in Wales, 1840–1886*. (Studies in Welsh History, number 6.) Cardiff: University of Wales Press; distributed by Books International, McLean, Va. 1991. Pp. xvi, 267. \$39.95.

In the first half of the nineteenth century Wales became one of the centers of European industrialization, and it is not surprising that the social transformation should have been reflected in political unrest. The years from 1838 to 1844 in particular were marked by such recurrent protest and violence that English commentators freely speculated on whether they were witnessing the birth of a "nearer Ireland" that would require the same pattern of military occupation and coercion. These fears proved excessive, but Wales over the next half century would remain central to most British movements of liberal or radical agitation: campaigns to expand the parliamentary franchise and democratize local government, to advance the rights of organized labor, and

(most persistently) to secure the rights of dissenters by disestablishing the Anglican church and abolishing tithes. These campaigns inextricably bound the interests of dissent and liberalism together with the emergent cultural nationalism of the period to forge a Liberal hegemony that would dominate the country until the rise of the Labour Party in the 1920s.

All these campaigns have been frequently recounted, but Ryland Wallace offers a valuable synthesis of the various movements, effectively portraying the culture of dissidence that had become so powerful by mid-century, a culture in which the same journals and groups moved freely from their agitations against tithes and the corn laws to demands for expanding the suffrage or reforming public health and education. In each case, the same tactics and rhetoric were inevitably applied, with varying degrees of suitability or success. The book presents an important discussion of the nature and limitations of the various reform groups, most of which drew heavily on the Anti-Corn Law League for their inspiration; it is especially useful in its analysis of the ties between the Welsh groups and their English models or progenitors. One of the best chapters concerns women's rights, a topic that frequently gets lost in general accounts of Welsh struggles of labor unions and radical dissenters. In every case, Wallace shows an impressive grasp of contemporary sources, including the flourishing Welsh-language press that played so pivotal a role in all these controversies.

The book can be recommended enthusiastically as a case study of the workings of local political societies in Victorian Britain, but a number of problems should be raised. Wallace underestimates the American influence on the various movements, most evidently in the area of the political meaning of religious dissent. Welsh radicalism would have been a slender tradition without the enormous fillips provided by successive religious revivals, above all that of 1859, and these all tended to reflect recent transatlantic events. The American Civil War had a comparably transforming and invigorating effect on every facet of Welsh Liberal culture.

This comment apart, the book is a solid contribution to the thriving field of the history of Victorian Wales. Its usefulness is marred somewhat by an undistinguished index.

PHILIP JENKINS
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JEREMY MORRIS. *Religion and Urban Change: Croydon, 1840-1914*. (Royal Historical Society Studies in History, number 65.) Rochester, N.Y.: Boydell, for the Royal Historical Society. 1992. Pp. ix, 236.

English men and women concerned about the state of organized religion at the turn of the present century saw the situation in terms of crisis. Jeremy Morris, in his worthwhile study of church and society in Victo-

rian and Edwardian Croydon, examines this perception in that particular suburban community and investigates the nature and extent of institutional and attitudinal change there over the course of seventy-five years.

What Morris discovers, if not particularly surprising, is nonetheless worth consideration. As Croydon's population grew with the influx of migrants from London in the mid-Victorian period, its increasingly middle and lower-middle-class inhabitants shaped a society in which church and chapel, and the voluntary associations they spawned, played a central role in the life and, more specifically, the governance of the town. The boundaries of authority were blurred for a couple of generations. Church business was parish business; parish business was the business of an elite that in time came to include Nonconformists as well as Anglicans, although the latter dominated the local establishment through much of the century.

Yet by 1900 all this had altered. Dissatisfaction with the inefficiencies of parochial government, coupled with the political opportunities afforded by an expanded electorate, encouraged—indeed, all but compelled—changes that would sharply curb the general authority of religious institutions, pushing them to the margins of political power. A parish council replaced the several increasingly ineffectual bodies, the most prominent being the local board of health, that had dominated governance at mid-century, and whose membership of church and chapel worthies had reflected the degree to which the lines between religious and secular authority were all but invisible.

In the course of his study, Morris highlights a variety of issues in a way that contributes to a clearer perception of the role of organized religion in Victorian England. He discusses the rivalry between Anglicans and Nonconformists, always there, yet at least in Croydon not as acrimonious as one might have expected. He devotes considerable attention to voluntary associations and to the limits of voluntarism as a method of local governance. Voluntarism afforded clergymen a major role in the affairs of the community.

When voluntarism was superseded by the centralized, secular instruments of state intervention, the clergy lost its power. The failure of voluntarism not only deprived the church of power; as Morris correctly concludes, it undermined "the ability of the church to cast itself as the indispensable social and moral binding force which the classes which principally attended and supported it hoped it would be" (p. 140). The consequence was that, by the end of the century, religious activity and the word "religious" itself were being redefined; as Morris points out, the word "was reduced from an all-embracing term . . . which expressed an entire culture . . . to a specialist description of a particular way of life or approach to morality" (p. 157).

Morris makes useful observations of this sort more than once, particularly in his later chapters on volun-

tarism and the changes that came with incorporation. Yet this study of parish matters suffers from the kind of parochialism that results when a dissertation is transformed too rapidly into a monograph. Although Morris considers briefly the work of other historians such as Stephen Yeo, Hugh McLeod, Jeffrey Cox, and C. G. Brown, one has the sense that this material was added as an afterthought. More disappointing is Morris's failure to make any use at all of Charles Booth's magisterial survey of religion in London at the beginning of the twentieth century, the seven volumes that concluded his *Life and Labour of the People of London* (1889–1902). The consequence of these limitations is an inability to apply the insights Morris has provided to a larger understanding of religion's role in the life and thoughts of Edwardian men and women.

STANDISH MEACHAM
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JAMES R. KINCAID. *Child-Loving: The Erotic Child and Victorian Culture*. New York: Routledge. 1992. Pp. xi, 413. \$35.00.

James R. Kincaid has written a delightful book sure to offend many readers, some of whom will be historians. Kincaid is evidently a smart, witty, and humane man who has written a powerhouse of a book, although one whose energy ultimately will be used most directly by literary critics rather than by historians. Nevertheless, those of us interested in the Victorians, in the history of the family, in the social and cultural constructions of sexuality, or in public policy issues and the criminalization of "deviance," will ignore this book at our peril.

Among other things, the book is a meditation on adulthood and childhood and how the two statuses are inevitably related in a sexualized way; not merely because it takes sex (or it used to) to produce children. More importantly, in a culture that increasingly defines people through their sexual conduct, the sexual Rubicon is the one children cross in becoming adults. Adults, we like to think, are sexual beings; children are not. Thus, Kincaid's argument is construed not only in our casting of sexuality but also in our casting of the child. Whereas pedophiles are marginalized and thrust in the role of "deviants," pedophilia itself is created by an eroticized discourse that creates a space for it. Simply put, the local news requires an ample supply of child molesters to fill its air time.

For Kincaid, making the reflexive move of placing himself (and us, his dear readers) within the text is not merely a rhetorical strategy (yes, I hear the groans of the lit-crit crowd over that adverb) but is central to his argument. We are the people who have created the institutions we profess to abhor. Thus, for Kincaid, an exploration of the Victorians is purpose-

fully present-minded: "I am less interested in reconstructing the past than in examining what our methods of reconstruction might tell us about our own policies . . . The past does not exist as a solid ground from which to assess the present; the past is there for us in reference to present needs" (p. 4). We see that we have constructed the Victorians as repressed so that we can congratulate ourselves on our own emancipation. Similarly, we insist that children are children so that we can be confident that we are not children, but rather that glorious, sexy Other, adults.

How successful is Kincaid in executing this ambitious project? His book is an intellectually flirtatious one, presumably on the principle that flirtation leads to fertility. It is as dazzling, alluring, sophisticated, playful, and seductive a work of scholarship as I have seen in a long time. But sometimes flirtation leads not to fertility, but only to desire, a consummation devoutly wished for but never achieved. Many readers, especially historians, will find it so.

The premise of the book is that we emancipated moderns and our Victorian ancestors are deeply implicated in each other. The problem is that the book lacks a *corpus callosum* to connect the two spheres of chronological experience. Kincaid does not make as clear as historians would wish why he selected the Victorians for his comparative lens, if only because he loses interest rather quickly in the Victorians. It is us he really cares about. The historian, however, hopelessly duped by the notion that effects must have causes, will want to see the connective tissue linking the two periods. If the eroticized child began in the nineteenth century, as opposed to another era, we would want to know why, where we have gone with the concept, and where we are going with it now. Consequently, those troubled by these issues will find parts of the book problematic, however scintillating the overall project.

The least successful section is that which directly reads literary texts, in which we follow the author into a long and tendentious descent into the Neverland of deconstruction, where his sense of humor disappears, a bad sign in so witty a book. We wonder if it is not only the reader who gets lost. The selections on *Alice in Wonderland*, *Peter Pan*, *David Copperfield*, and even *Tess of the d'Urbervilles* make good sense, but eternally cute Holden Caulfield is thrown in without explanation. To say that Kincaid collapses the 1950s into the dim mists of the Victorian past is inadequate, even by his own lights. Kincaid earlier deconstructed our own vision of Victorians, showing how bamboozled we have been by them. If we are to think the 1950s Victorian in the truest historical sense, then Kincaid has persuaded us that they were more honest and sensible than we are. But this is not the reading Kincaid gives either *Catcher in the Rye* or any of the other novels on which he focuses.

Are we inside or outside the Victorians? If 1951 cannot be considered Victorian, is it now? Or is poor Holden to be lost outside of time? Why does public

fascination with, horror over, and titillation by pedophilia seem to have gotten worse, more dishonest, more extreme, and more central to our shared discourse? As historians we would like to make connections between literary texts and empirical reality, convinced as we still are that the latter both exists and counts. And it seems also to count to Kincaid, whose long exegesis on the McMartin trial could easily be extended to the Waco disaster and the Michael Jackson case, had he known enough to wait before writing this book. But future historians will have to be motivated, as they will be by this book, to identify those connections.

Rather than smugly chiding Kincaid for not being a historian, it is perhaps more responsible and productive to ask how this important book and its rich insights might shape our own research agendas. His discussions of power and gender seem likely to prove especially fruitful: "Denying the child access to sexual feeling is also a way of maintaining unchallenged what are clearly traditional views of gender, family, and authority" (p. 375). He notes that the widely accepted notion that abused children are likely to become abusers themselves presents us with a conundrum we choose to ignore: if most victims are female, how come most abusers are male? Although he is not explicit on this point, the reader concludes that making males the only acceptable perpetrators reifies notions of male power and sexual omnivorousness. It underscores the "boys will be boys" excuse while simultaneously requiring that children, like women, find protection from males in other males.

Ultimately, the pedophile culture that Kincaid makes explicit plays a role similar to that of the rape culture in the intimidation of women. The more the world outside the home is demonized, the more the home becomes the requisite sanctuary. Women and children alike are driven into the home, according to this potential line of analysis, from fear of what lurks without, leaving them all too often under a protection that is experienced as control. Wives and children were legal possessions of adult males until rather recently. What we have here may be an internalization of control, as fear reinforces the practical, if not legal, dependence of women and children. But this is only one of the many potential lines of inquiry suggested by this important and provocative book, likely to be discussed by historians and literary critics alike.

JUDITH S. LEWIS
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GEOFFREY ALDERMAN. *Modern British Jewry*. New York: Clarendon Press of Oxford University Press. 1992. Pp. ix, 397. \$89.00.

Geoffrey Alderman's history of the British Jewish community from the mid-nineteenth century to the present is in every way an improvement on its only

predecessor, V. D. Lipman's *History of the Jews in Britain since 1858* (1990). Written in a lively, engaging manner, it casts a critical eye on the intracommunal strife that Lipman and other filiopietistic historians passed over in silence or treated in an apologetic fashion. This is most apparent in the three central chapters on East European immigration and the upheaval in communal life it caused. Whereas Lipman glossed over the disruptive dimensions of East European settlement, ignoring in particular the dismay, even fear, it aroused among the leaders of the established community, Alderman explores the reactions of the anglicized leadership at length. His is also a more comprehensive work than Lipman's. In addition to a greater wealth of detail, it includes the first substantial treatment of events and trends in the interwar period and the half-century since World War II.

A political historian, Alderman is at his best in describing the communal politics of British Jews, especially the intense struggle in the late nineteenth and early twentieth centuries between the old, assimilationist establishment and the new arrivals for control of communal institutions and leadership. This conflict was expressed in confrontations on multiple fronts, which the author describes in vivid and often partisan terms. In his words, "oligarchy was confronted by democracy; laxity by orthodoxy; political conservatism by social radicalism; synagogal centralism by the independency of the *chevrot*; the numerical dominance of London by the jealous independence of provincial Jewries; the institutionalized charity of the Boards of Guardians by the communal self-help of the friendly societies" (p. 209).

Alderman's animated treatment of intracommunal politics leaves no doubt about his own involvement in recent controversies over communal governance, in which he was a vigorous critic of the claims of the chief rabbinate. His interest in intra-Jewish strife is not balanced, however, by careful attention to themes of equal importance in the development of British Jewry. He devotes little space to questions of acculturation, integration, and secularization and the demographic decline due to intermarriage, conversion, and indifference that came in their wake. He has virtually nothing to say about the causes or pace of the embourgeoisement of the immigrants and their descendants. He addresses neither the historiographical debate about the class character of immigrant workers, an issue critical to discussions of Anglo-Jewish economic mobility, nor the debate about whether Anglo-Jewish emancipation was contractual, as was emancipation in most European states, or unconditional and thus relatively unique. And he fails to mention, let alone explain, the unprecedented centrality of Israel on the communal agenda—and in individual consciousness—since 1967.

In addition to its imbalance, Alderman's account is marked by a lack of interest in the "whys" of British Jewish history. With the exception of his treatment of

Jewish political behavior in parliamentary elections, this volume is long on description and short on analysis. This can be linked in large part to Alderman's failure to situate the history of British Jewry in the larger context of either the history of other minorities, religious and ethnic, in Britain, or the history of Jewish communities in other Western societies. The self-referential, inward-looking character of his account makes it difficult to know the extent to which the central features of British Jewish history were shaped by the setting in which they unfolded or, alternatively, were shared by Jewish communities in other liberal societies. In failing to contextualize his treatment of British Jewry and thus to link its history to broader issues of historical interpretation, Alderman ironically replicates the chief shortcoming of the older, amateur tradition of Anglo-Jewish historical scholarship of which he is so critical.

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G. R. SEARLE. *Entrepreneurial Politics in Mid-Victorian Britain*. New York: Clarendon Press of Oxford University Press. 1993. Pp. viii, 346. \$59.00.

G. R. Searle, a historian with a well-established reputation in *fin-de-siècle* British political studies, turns in this major work to the mid-nineteenth century and to a subject, the middle-class entrepreneur, that has been the target of much attention in the last decade. What we are given is not, however, a rehash of the now familiar thesis expounded by Martin Wiener (*English Culture and the Decline of the Industrial Spirit 1850–1980* [1980]), the debate over the emergence of gentlemanly capitalism and the social and political hegemony of the traditional ruling elite. Instead we have a straightforward account of parliamentary and extraparlimentary politics and of the ideas, issues, and arguments employed by radical businessmen and their allies. It is a traditional study of political discourse gleaned from correspondence, newspapers, and parliamentary speeches.

It would be wrong, however, to dismiss this book as merely an exercise in old-fashioned political historiography. Searle suggests that the past generation of scholars has in fact been overly occupied with the social composition of the Victorian system, with establishing large categories of class and viewing politics merely as a field for their expression. There is, he argues, a need to return to the issues that exercised the attention of influential individuals of the commercial middle class in order to reexamine the complexity of their opinions and aspirations. This setting aside of social context is a limitation that will have its critics, but it does offer a refreshing and important contribution in the hands of a historian so thoroughly at home in the extensive primary sources.

This work passes over some very familiar ground,

the great political controversies of the classic middle period of the Victorian system. What gives Searle his angle on all this is his concern to tease out and follow the particular point of view of those whom he calls the "entrepreneurial Radicals" (p. 16), those who—often themselves businessmen—brought to parliamentary life an insistent demand that the "rational" criteria of the market be brought to bear on all operations of government. From the repeal of the corn laws, through the various reforms of the fiscal and administrative systems, to education and labor relations, he presents this capitalist perspective (or, often, perspectives). One virtue of this study lies in the way it reveals the conflict and confusion among the middle-class radicals as they struggled to discover the appropriate application of economic law. Their differences over income tax, civil-service reform, or limited liability belie easy textbook generalizations about the unity of commercial middle-class opinion.

Any study such as this necessarily shapes itself around Richard Cobden, the hero of parliamentary political economy and the pivotal figure of Searle's text. In a sense the book is an extended investigation of Cobden's often unsuccessful attempt to inculcate a purer form of entrepreneurial radicalism in his followers than they were prepared to accept, his relations with leading establishment politicians, and his posthumous influence. Searle persuasively argues that we are too ready to view the period in terms of the anticipated failure of middle-class political economy after the 1870s. Such Whiggish history needs the corrective, he believes, of a closer examination of the period in its own right and an appreciation of the fact that entrepreneurial radicalism was tougher and more pervasive than such an overview leads one to expect. Getting "value for money" in every aspect of government activity was a middle-class demand that did not lose its relevance as quickly as is sometimes supposed even if Cobden's personal hopes for a revolutionary assault on landed privilege never materialized (p. 293).

A byproduct of Searle's perspective is his ability to illuminate some familiar issues of political historiography, most importantly William Ewart Gladstone's relationship—via his brother Robertson—with the entrepreneurial radicals; larger issues and connections involving the shifting social environment of the four decades of Searle's story, however, are left for others to follow. What he does offer is a close look at the discourse of a well-defined group whose ideas and arguments, although they themselves were rarely in the prime positions of political power, ran through the whole fabric of the mid-Victorian political world. This is a well-controlled, deeply informed study characterized by balanced and judicious argument. It is a model of its kind and will be widely consulted as a sound source on an important subject.

H. L. MALCHOW
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CHARLOTTE MACKENZIE. *Psychiatry for the Rich: A History of Ticehurst, Private Asylum*. (The Wellcome Institute Series in the History of Medicine.) New York: Routledge. 1992. Pp. x, 234.

At the height of its reputation in the 1870s, Ticehurst Asylum in Sussex charged an average annual fee of around £450–500. More than twenty patients kept private carriages on the asylum grounds, which eventually boasted facilities for cricket, bowls, croquet, golf, archery, fishing, tennis, cycling, and even hunting. Over the years, its patients included the Countess of Durham, the Earl of Carlisle, and an Egyptian prince. After 1893 a French chef prepared their meals. Ticehurst was clearly exceptional, even among the private asylums catering to affluent families in nineteenth and early twentieth-century England. To Charlotte MacKenzie's credit, she makes no attempt to present Ticehurst as typical of developments in the private sector of the asylum business during these decades.

Nor does she offer her readers a script for "Lifestyles of the Rich and Crazy." Eschewing largely futile debates over the question of whether its patients were really mad, MacKenzie provides a sober, exhaustive study of Ticehurst's history under the management of four generations of the Newington family, from 1792 to 1917. In the process she tests some of the generalizations proposed by historians of psychiatry and tries to fill some spaces in the picture of asylum care they have painted. She persuasively demonstrates how private madhouses, developing before the network of public psychiatric institutions emerged in England, challenge the social-control hypothesis for explaining the proliferation of asylums in the eighteenth and nineteenth centuries. Although she offers no single alternative thesis, she—like Roy Porter—finds part of her explanation in the consumer boom of the late eighteenth century and the concomitant expansion of service industries; thus, "the trend towards confinement away from home can be seen as part of the aspirations to gentility which accompanied increased spending power" (p. 20). She is particularly interested in exploring the reasons why affluent families chose to send relations to a private asylum instead of providing care at home, a topic she comes back to repeatedly.

The tendency to retread the same ground is the book's only substantial weakness. MacKenzie proceeds in largely chronological fashion, surveying the state of the asylum under each generation of managers, so that the same issues are examined again and again. For example, she reiterates the complex relationship between moral and medical/mechanical forms of therapy, and, although she underscores the shift in therapeutic emphasis over time, the points she wishes to argue from these changes tend to disappear in the highly detailed information about patients, attendants, fees, lunacy legislation, and official inspectors. A separate chapter solely devoted to thera-

peutic controversies during the course of the nineteenth century would make MacKenzie's themes more readily accessible to the reader.

This volume developed from MacKenzie's doctoral dissertation at the University of London. When she occasionally shifts focus from the particular to the general and tries to apply her findings to broad social trends in Victorian society outside the asylum, she seems hesitant and derivative. Within her own sphere of expertise, however, MacKenzie furnishes a valuable study, based on careful archival research, of the attitudes and assumptions that shaped treatment at Ticehurst for more than a century.

JANET OPPENHEIM
American University

JOSEPH MCALEER. *Popular Reading and Publishing in Britain, 1914–1950*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1992. Pp. 284. \$62.00.

Joseph McAleer provides fresh information about the dramatic growth of the reading public in the late nineteenth and early twentieth centuries in this monograph, based on his doctoral dissertation. Eschewing the theoretical approaches of cultural studies, McAleer grounds this empirical work on research in business archives. Although he concentrates on the economics of the publishing industry, he has also read widely in the popular magazines and fiction of the period, an often neglected source. He also interviewed former employees of the major publishing firms, whose provocative quotations enliven the text.

McAleer demonstrates how the demand for light fiction accelerated during the interwar period, when retail prices declined and the number of published titles increased markedly. New editions of adult fiction doubled during the 1920s, eventually reaching over 2,800 titles per year in the mid-1930s. Libraries and other lending facilities also expanded dramatically, tripling the number of books in stock from 1924 to 1949. The industry restructured itself, adapting economies of scale and innovative marketing techniques that resulted in substantial savings. Penguin Books, founded in 1935, sold paperbacks in chain stores such as Woolworths and even installed a vending machine at Charing Cross Road. Books became a disposable commodity not unlike magazines. Although World War II created serious problems for publishers, including the infamous raid on Paternoster Square in 1940 that destroyed five million volumes, reading prospered during the war and the number of publishers actually increased.

McAleer devotes a substantial portion of his book to case studies of major publishers. Mills and Boon, founded in 1908, only began to focus on romantic fiction during the 1930s, when the reputation of their imprint gradually transcended the fame of their individual authors. The company became a brand

name for an identifiable form of romantic fiction. Based on his close reading of several texts, McAleer argues that this fiction embodied principles of sexual purity that, like other genres in popular culture, often manifested a profound social conservatism. Yet he also shows how individual heroines exercised a level of personal autonomy that might be considered mildly feminist.

In perhaps the most vivid chapter of the book, McAleer narrates the story of D. C. Thomson and Company, publishers of five immensely popular boys magazines: *Adventure* (1921–63), *The Rover* (1922–73), *The Wizard* (1922–78), *The Skipper* (1930–41), and *The Hotspur* (1933–81). Its magazines detested by George Orwell but embraced by the public, Thomson kept in touch with its young readers by sending representatives throughout the country to interview boys as they left school, a practice known as “speering” (p. 180). The chairman of the organization, called by some “the newspaper Mussolini of Dundee” (p. 162), championed patriotism, discouraged sex, and perpetuated racial stereotypes, especially about Africans and Asians. Unlike the Religious Tract Society that, as McAleer demonstrates, allowed its once-dominant publications such as *Boys Own Paper* and *Girls Own Paper* to become hopelessly dated, D. C. Thomson maintained its market share by responding flexibly to changes in public demand.

McAleer has written a valuable book on an important topic. Although his prose frequently lapses into clichés, he demonstrates the utility of solid archival research in a field still dominated by theory.

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GARY S. MESSINGER. *British Propaganda and the State in the First World War*. New York: Manchester University Press; distributed by St. Martin's. 1992. Pp. x, 292. \$59.95.

Gary S. Messinger's subject is topical in light of the extreme visibility of government information and disinformation in recent wars; I am among those historians who welcome such relevance, when supported by good scholarship. The book is principally organized under the names of a series of men who played significant roles in promoting the British cause from 1914 to 1918. The strength of this format, which is the book's only claim to originality, is in highlighting the variety of those who contributed to the burgeoning propaganda machine, and Messinger combines it with something of a thematic approach; for example, the chapters bear titles such as “Recruiting the Intelligentsia: Charles Masterman,” and “Violating the Political Code: Lord Northcliffe.” The successive chapters follow a rough chronological order, with some inevitable overlap.

Messinger's thesis is confusing. He gives a convincing picture of the immediate drop in ethical stan-

dards in government propaganda at the outbreak of war, citing the infamous Bryce commission on atrocities, and recognizes a steady further decline from there on. Yet in his concluding chapter he backs away from this critical approach and takes the view that it was too bad that the propaganda machine was dismantled when the war ended. Although he may be right that the fascist and Nazi propaganda machines had the jump on the democracies in some respects, he does not tackle the question of what would have been the line taken by successive British governments of different parties, nor does he look at what were the chances, given the record, of their maintaining credibility. He rightly draws attention, however, to the tragic retribution for government fabrication of atrocity stories in 1914–15, which contributed to disbelief of the only-too-true accounts of the treatment of Jews in the 1930s.

If Messinger's ethical analysis is weak, any political analysis is virtually lacking. Because Messinger's avowed justification for adding another work to the growing literature on propaganda in wartime is to examine the personalities, motivation, and anxieties of the individuals who wielded the new tool (p. 2), his failure to discuss—in some instances even to mention—their party political conditioning is critical. He could have taken as a starting point Irene Cooper Willis's *England's Holy War* (1928). Her study showed Britain taken into the war by a Liberal Party ideologically identified with internationalism and peace, which had then to justify itself by making the war into a righteous campaign against unspeakable violations of humanitarian principles, so finding itself in uneasy alliance with conservatives whose concern was simply with a more energetic prosecution of the war.

The lack of depth in Messinger's commentary becomes more explicable when we look at the methodological flaws. There is nothing inappropriate in the use of published memoirs of the propagandists. But all autobiographical material, and much biographical material, has to be approached with care and with the certainty of bias. Messinger's referencing is not reassuring. For almost every chapter he cites a major source or (rarely) two, adding some such phrase as “from which most of the biographical details in this chapter are taken” (p. 259, n. 1). A twenty-page chapter on R. W. Seton-Watson and Henry Wickham Steed boasts a total of only three reference notes, so that we are presumably to trust the word of the protagonists—as the author does—not only for direct quotations but also for the actions, motivations, rationalizations, and reported conversations of both themselves and everyone they had dealings with. Likewise, we are left with these sources for the facts themselves, which Messinger's own subject should have taught him are by no means immune from distortion and outright falsification. Although most chapters are indeed more fully (although never adequately) documented, and a few include forays into primary sources, Messinger throughout shows an

absence of critical judgment of his sources. Messenger's approach might have been appealing and useful, at least at the undergraduate level, but as a scholarly role model the book is unacceptable, so even this use will be limited.

JO VELLACOTT
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DONALD RICHTER. *Chemical Soldiers: British Gas Warfare in World War I*. (Modern War Studies.) Lawrence: University Press of Kansas. 1992. Pp. x, 282. \$35.00.

Outrage over German introduction of asphyxiating gas at Ypres in April 1915 prompted the Allies to retaliate in kind. British chemical warfare was entrusted to the Special Brigade, an elite section of the army recruited by an energetic engineer officer, Major Charles Foulkes. Initially restricted technologically to the use of chlorine discharged from cylinders, British scientists strove to devise more toxic forms of gas and new apparatus for delivering it. Until nearly the end of the war, Foulkes remained optimistic that gas alone would break the deadlock in the west but, in reality, gas was anything but effective or reliable. After the war, Foulkes, dismayed at the short shrift given to the Special Brigade's contributions in the official history, published his own account entitled *Gas! The Story of the Special Brigade* (1934). Foulkes provided invaluable first-hand experience, but he was much too concerned with justifying the actions of his brigade to view it in an unbiased and objective light.

In this study about the Special Brigade and its commander, Donald Richter provides much additional information as well as a balanced perspective. He investigates in detail the composition, mission, and activities of the Special Brigade and, in the tradition of John Keegan, places greater emphasis on the individual experiences and emotions of the men in the unit. His chapters describing the technical problems and the gas operations at Loos and the Somme are fascinating and give us a more complete understanding of these battles. Richter observes that the Brigade kept Britain in the forefront of gas warfare by its tireless efforts to improve old techniques and experiment with new ones. He discusses with clarity and care the controversy over the relative value of two competing delivery systems, the cylinder and the Livens projector (which fired gas bombs). The question was never resolved, for it was not possible to obtain reliable statistics.

A consistent thread running through this book is the futility of British chemical warfare. Improvements in defensive equipment far outpaced offensive gas capability. Moreover, the vast majority of commanders were obstructive, believing that gas was more of a hindrance than a help. Lastly, the uncontrollable variable of ideal wind conditions made it highly improbable that gas emissions could be pre-

cisely synchronized with an infantry assault. As for timing the infantry attack to a favorable wind, it was impossible to maintain both an extensive barrage and the infantry in a state of readiness for an indefinite period. After the Somme, gas became increasingly divorced from specific offensives and used more and more as a routine harassment of the enemy.

Despite the disappointing results of chemical warfare, Richter pays high tribute to the men of the Special Brigade. They were, he points out, innovative, hard working, and courageous, carrying out in a professional manner what was demanded of them, frequently under the most trying circumstances. The author's opinion of Foulkes is nearly as complimentary. While conceding that Foulkes was obstinate, brusque, and at times difficult, Richter sees Foulkes as an active, self-confident, and capable leader, undaunted by physical dangers or Herculean challenges, and concludes that he was the right man for the job.

Richter's inference that he is the first to demolish the popular myth that gas was an unnecessarily cruel weapon is an overstatement. This is a minor quibble, however, for his study is elegantly written and meticulously researched. Richter has mined numerous private collections and unpublished diaries as well as mastered the most recent books and articles pertinent to his subject. This well-informed study is certain to remain the definitive work, at least for the foreseeable future, on British gas warfare in World War I.

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STEPHEN BROOKE. *Labour's War: The Labour Party during the Second World War*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1992. Pp. xiii, 363. \$85.00.

This work, the first full-length study of the Labour Party during World War II, takes issue with the widely accepted view that the Labour Party, as a consequence of its membership in the wartime Coalition government, shifted its goals from the building of socialism to the reform of capitalism. Admirers and critics of the Labour governments of 1945–51 alike have viewed their policies as simply the implementation of reforms agreed on in the course of the war by both the Conservative and Labour parties. Likewise, this wartime consensus is seen as extending through the 1950s, the era of "Butskellism," when the Conservative, R. A. Butler, and the Labourite, Hugh Gaitskell, jettisoning ideological concerns, employed identical Keynesian tools in their efforts to control the British economy.

Stephen Brooke buttresses his argument that the Labour Party did not lose its distinctive ideological outlook with material from an admirably wide range of sources: Labour Party papers, Cabinet documents, private papers, and newspapers. Although the author

focuses on developments at the center of British politics, his use of the minutes of local and regional Labour parties and councils gives evidence of the pressures from their constituency to which Labour leaders were forced to respond.

Brooke demonstrates conclusively that Labour members of the Coalition were relentless in pressing for reforms that they had championed in the prewar years. The effectiveness of these efforts, however, was mixed. Their hopes that wartime needs would lead to measures such as nationalization of the coal mines proved vain. In the social area, however, they had considerable success in influencing a series of White Papers on postwar planning, the most important of which was the famed Beveridge Report recommending a thorough overhaul of the system of social insurance. Given the fact that the large Conservative majority in the House of Commons made legislative advances along socialist lines almost impossible, these achievements in the area of postwar planning were the best that could realistically be expected.

Brooke concedes that a transformation did occur in the Labour Party's socialist ideology in the course of the war, and he outlines this change clearly. Public ownership of industry ceased being regarded as an end in itself and became only one means among many to achieve an efficient managed economy. Indeed, some socialist thinkers began to argue that with other tools such as fiscal controls available for regulating the economy, the extent of public ownership needed might be limited. The author downplays the influence of the non-socialist John Maynard Keynes in producing this shift, arguing that it was largely attributable to pressures from within the party itself. This view is not entirely persuasive. Keynes's teachings on the management of the economy had to be accepted or rejected, and the Labour Party, like the Conservatives, accepted them, albeit as part of a distinctively socialist program.

Although something of value was doubtless lost in this shift from ethical socialism to the less-inspiring structural approach to the economy, the achievements of Labour leaders during the war remain impressive. They succeeded in focusing attention on proposals for sweeping changes in the areas of health, unemployment, and old-age pensions that were largely achieved in the postwar years. Measured by the record of the Labour Party following World War I, when Labour leaders were unable to maintain the wartime gains of workers, let alone to extend them, the accomplishments of Labour members of the Coalition in preparing for the postwar world seem admirable.

Whereas Brooke does not completely succeed in demolishing the view that the British people and the political parties representing them reached an unusual degree of consensus concerning the direction in which they wished their society to move in the postwar years, he does demonstrate how large the Labour Party's contribution was to that consensus, and how

consistent that contribution was with Labour's traditional goals. However partial their acceptance of his thesis may be, historians of twentieth-century Britain are fortunate to have this solid careful study of the Labour Party during a hitherto neglected period.

CATHERINE ANN CLINE
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JOHN BAYLIS. *The Diplomacy of Pragmatism: Britain and the Formation of NATO, 1942-1949*. (American Diplomatic History, number 5.) Kent, Ohio: Kent State University Press. 1993. Pp. xi, 194. \$35.00.

John Baylis is the author of numerous studies on British defense policy and foreign relations in the mid-twentieth century. In this short book, he turns his attention to Britain's role—more specifically, to the role of Ernest Bevin—in the formation of NATO. The author's prose is clear, and so is his position, which is that of “depolarization.” Although his study is not directly concerned with the origins of the Cold War, he argues strongly that post-World War II international relations involved much more than the Soviet-American confrontation, whether it be viewed from a traditional (Joseph Stalin's fault), revisionist (America's fault), or postrevisionist (both guilty) standpoint. In short, Baylis believes that other states also had major roles in shaping events, beginning—and in this book ending—with Britain.

One advantage of this study is that it begins with wartime planning for the postwar era, thus giving an uncommon continuity to the evolution of postwar policy. Hindsight, as Baylis points out in his conclusion, reveals British statesmen as inhabiting a dream world of the future, in which Britain was projected to emerge as powerful as ever. The apparent reality at the time, however, was that Britain truly carried a lion's share of the wartime burden and logically could be assumed to do so in the future, except, perhaps, by the Treasury, which knew better. Just what the future portended was never clear, however, and that was part of the problem, with the Foreign Office on one side preoccupied with the potential reemergence of Germany, and the chiefs of staff on the other side already concerned with the growing menace of Russia. Bevin thus had some key decisions to make after becoming foreign secretary in mid-1945.

Baylis is not blind to Bevin's misperceptions, but his account on the whole is laudatory, concluding that Bevin's role in the formation of NATO was of considerable importance. Bevin has of course been criticized from the Left for being insufficiently cooperative toward the Soviet Union, but Baylis believes that Bevin was more than willing to work with Moscow if and when Moscow gave clear evidence of similar intent. A favorable interpretation of Bevin is not exactly revolutionary (see Alan Bullock's biography, *Ernest Bevin: Foreign Secretary, 1945-51* [1983]), but Baylis is helpful in placing Bevin in the larger context

of postwar constraints on British foreign policy. Unfortunately, the limited scope of the book precludes more than the briefest consideration of the role of such issues as Indian partition, Palestine, Czechoslovakia, or even the Berlin blockade.

The book, which is thoroughly grounded in British and American documents, is usefully supplemented by five key documents (pp. 131–63), including the North Atlantic Treaty, and a brief selected bibliography that unfortunately includes only books and no articles. This work is part of a general series entitled “American Diplomatic History” edited by Lawrence Kaplan; while indeed it will interest students of American foreign policy during the birth pangs of NATO, readers should be advised that its focus is Britain.

BRITON C. BUSCH
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JAMES TUCK-HONG TANG. *Britain's Encounter with Revolutionary China, 1949–54*. New York: St. Martin's. 1992. Pp. xiii, 264. \$65.00.

As James Tuck-Hong Tang reminds us in this book, diplomatic recognition and diplomatic relations are not the same thing. In early 1950, Britain was the first major Western power to recognize the new People's Republic of China, yet not until late 1954 were formal relations opened, and not until 1972—well beyond the scope of this book—did the two countries exchange ambassadors.

Tang recognizes that the question of recognition did divide Britain from its Western allies, but he warns against overemphasizing the split; before the Korean War, even the United States was not so much opposed to recognition in principle as afraid that premature action would lead to problems. In Britain, Tang shows that Whitehall was the target of strong arguments and strong lobbying in favor of recognition. The Foreign Office saw the new government as the *de jure* ruler of the country and maintained that recognition was politically prudent, allowing Britain to maintain a foothold in China, while business interests had no desire to see their position there weakened. Several Commonwealth nations, particularly India, urged London to recognize Beijing. The opposing arguments were comparatively weak: they came from some Commonwealth nations that were unwilling to be out of step with the United States; from the Colonial Office, which worried about Chinese subversion in Malaya; and from the chiefs of staff, who worried about communist expansionism. Yet even these groups, seeing the Soviet Union as the real enemy, understood that China ought to be kept out of Moscow's embrace.

While seeking to follow an independent Asian policy, both Labour and Tory governments put the maintenance of the Anglo-American alliance at the heart of their foreign policy. As American views

hardened after June 1950, Britain's balancing act became more difficult, particularly when, later that year, British troops found themselves facing Chinese soldiers. Tang shows that Beijing was in no hurry to open formal diplomatic relations, insisting that London first meet certain conditions. British business interests in cities like Shanghai and Tianjin were being frozen out, and by 1951 most British concerns on the mainland had disbanded.

Nonetheless, British diplomatic and consular representatives remained in China, receiving conventional diplomatic courtesies even during the Korean War. Finally, at the Geneva Conference of 1954, British and Chinese representatives reached an agreement for formal relations, albeit for the next eighteen years they would remain at something less than the ambassadorial level.

Tang argues both that China policy was not, in fact, a major cause of friction between Britain and America, and that throughout this period, China's practice of revolutionary diplomacy was tempered with a strong dose of pragmatism. Perhaps we could have had a bit more on the issue of Taiwan; although Tang mentions in passing Britain's maintenance of a consulate on the island, he says little of the complications it might have caused.

This is a very useful book, and the most detailed examination yet of Britain's China policy in these years.

NICHOLAS R. CLIFFORD
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GRÁINNE HENRY. *The Irish Military Community in Spanish Flanders, 1586–1621*. Dublin: Irish Academic Press. 1992. Pp. 208. \$45.00.

Gráinne Henry addresses a topic that deserves the attention of serious scholars. Beginning in the late sixteenth century, Ireland became an important source of mercenaries for continental states. These “Wild Geese” migrated by the thousands to serve in the armies of Catholic Europe, especially Spain. Henry focuses on the reasons why Irish soldiers migrated to Spanish Flanders (modern Belgium) from 1586 to 1621, the formation of an Irish regiment in the Spanish Army of Flanders, the establishment of an Irish military community, and the growth of a political identity in that community.

The book is the result of thorough research in primary sources in British and Continental archives and a fairly broad (although incomplete) knowledge of the secondary literature of the general field. Henry has concentrated on the migration of rural Irishmen, especially those professional swordsmen who historically had provided upstart Irish lords with the military means necessary to challenge Anglo-Norman or English rule. The large-scale departure of this Irish military class could only have had a notable impact on both the British Isles and the Continent during the Counter Reformation. Although Henry owes a schol-

arly debt to works such as Brendan Jennings, ed., *Wild Geese in Spanish Flanders, 1582–1700* (1964), and Micheline Walsh, *"Destruction by Peace": Hugh O'Neill after Kinsale* (1986), her work goes appreciably deeper. By incorporating the "Irish problem" into a broader range of issues—political, diplomatic, religious, and military—facing Europe in the three decades preceding the Thirty Years' War (1618–48), she, like Geoffrey Parker in *The Army of Flanders and the Spanish Road, 1567–1659* (1982), presents a detailed picture of the interplay of complex forces to explain why Flanders deserves the appellation "the cockpit of Europe."

If Henry's study merits criticism, it is for giving inadequate attention to the Irish military tradition that evolved in the sixteenth century. Since the arrival of hordes of Scottish "redshanks" (heavy infantry) under the MacDonnell banner in the first half of the century and the subsequent campaigns of Shane O'Neill in the 1560s, in which firearms played an important role, Irish warfare had combined shock power, mobility, and increased missile firepower. Although her emphasis is not on tactics and operations, Henry would have done well to have presented a more detailed background discussion of such matters. The book also suffers in places from poor copy-editing, and the readability of the text is marred by far too many block quotations. Overall, however, this book is a most valuable addition to the literature of early modern Irish history. One would hope that Henry continues to pursue this topic further into the seventeenth century.

J. MICHAEL HILL
Stillman College
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MARY E. DALY. *Industrial Development and Irish National Identity, 1922–1939*. Syracuse, N.Y.: Syracuse University Press. 1992. Pp. xv, 201. \$34.95.

Mary E. Daly is one of a number of revisionist Irish economic historians who have sought to temper the often heavily ideological and nationalist interpretations of Irish economic development. Her current study builds on her earlier *Social and Economic History of Ireland since 1800* (1981), where she argued that an independent government in nineteenth-century Ireland would not have brought major changes in its economic fortunes. In her new work she maintains that the modest and slightly confused economic achievements of an independent Ireland during the 1920s and 1930s suited the new nation's search for relative stability, although at the price of continued emigration.

Using a wide range of official and some private manuscript sources as well as extensive published material, Daly tells the story of the effort to forge an Irish nationalist economy in a nation committed to Catholic and agrarian values. The new Irish state

inherited an economy led by a cattle-exporting agriculture, a manufacturing sector dominated by food processors, and financial institutions integrated with those of Britain. It also inherited a civil service and elite that was suspicious of government intervention. Alternative visions of a protected and self-sufficient Ireland, of peasant proprietors and union-dominated industry, had been ably articulated by radical leaders but enjoyed little support with Ireland's first independent government.

The Cumann na nGaedheadh government of the 1920s preserved established economic ties with Britain, bore the onus of an overvalued currency, and paid a heavy burden of land payments to Britain. It clung to these policies despite the world economic crisis. When Fianna Fail came to power in 1932, it promised to end land payments, extend the drift toward tariffs to full-scale protectionism, promote industrial self-sufficiency, provide employment, and secure an adequate but frugal standard of living for all. Between 1932 and 1936 Ireland experienced an impressive rate of industrial development under a complex regime of tariffs, licenses, quotas, politically motivated decentralization of industries, and various subterfuges that hid the fact that a good deal of this industrial development was foreign-owned. Although Daly recounts innovative efforts to produce an Irish capital market, Ireland remained firmly committed to sterling and private control.

Whether this was an Irish industrial revolution, as Daly asserts, is debatable. In any case, Fianna Fail's version of the New Deal did little to solve the problem of emigration or the continued migration of the poor to the cities. By 1938 the attempted economic revolution had run up against reality and conflicting ideals. An Anglo-Irish trade agreement ended the economic war with Britain and confirmed the existence of a new Irish establishment consisting of cattle exporters, food processors, a protected manufacturing sector, and subsidized family agriculture. Daly argues that this conservative synthesis was not the failure of an Irish economic revolution but a reasonable response to economic, social, and political realities.

Throughout this carefully argued study, Daly provides a useful perspective by comparing the Irish experiment in economic nationalism with that of other nations. This saves her work from the insularity and romantic notions still too often associated with Irish history. My chief criticism of the book is with its sparse style. One often wants to hear more of the voices and rhetoric used to explain the positions taken by the protagonists. Yet its brevity makes this an excellent book to recommend to students. Specialists will find that Daly takes a number of controversial but well-defended positions in her argument, and that she emphasizes the relative continuity of Irish economic history.

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LARISSA TAYLOR. *Soldiers of Christ: Preaching in Late Medieval and Reformation France*. New York: Oxford University Press. 1992. Pp. xiv, 352. \$55.00.

Larissa Taylor's study is a handy review of the standard literature on late-medieval preaching and sermon form (part 1), a useful source for pre-Tridentine preachers' views on religion and society (part 2), and an interesting description of the gradual progress of heresy (and orthodox response to it) in France from the eve of the Protestant Reformation to the 1550s (part 3). Although she makes good use of secondary literature on the medieval art of preaching, Taylor has also done considerable original research on contemporary French sermons: 1,657 sermons by twenty-three preachers and contained in forty-three collections.

Students unfamiliar with the large body of literature on the medieval *artes praedicandi* in England, France, Germany, and Italy will no doubt find Taylor's synthesis time saving. She discusses preaching as event, the preacher's background and training, as well as sermon language, technique, and form.

More original is Taylor's use of sermons as a source for mid-fifteenth to mid-sixteenth-century French preachers' ideas regarding religion. She argues that Johan Huizinga's notion of the time before the Reformation as one of decay and dread that set the stage for the likes of Martin Luther and John Calvin is mistaken. Preachers' references to eschatology are far outweighed by themes such as the need for penitence and the imitation of Christ (p. 100). Furthermore, Taylor shows, "As in their treatment of God, our preachers emphasized the loving consolatory face of the Holy Spirit" (p. 106), and they "concentrate on Christ's earthly ministry, including his preaching, his manner of living, and the example he set for mortal men and women" (p. 107). The fifteenth century as reflected in sermons was a time of great optimism regarding human possibilities: "if man did everything in his power he would be saved" (p. 140).

Taylor also shows that French Catholic preachers in the years just before and after 1520 were not greatly alarmed by the pre-Protestant and Protestant heretics. They responded to criticisms of abuses within the church by insisting on "reform among both clergy and laity and a collective effort to 'restore discipline, ritual, and a [sic] mores' as the best way to respond" (p. 217). The initial response to criticism was to heed it. By the 1550s, however, that response had changed; the danger was now seen as so great that preachers called for a counterattack. Heretics were to be prevented from preaching, and if they would not return to the fold, they must be burned.

Most of Taylor's historical interpretation comes when she writes about religious ideas. Her examination of preachers' views of women and the structure of society, however, is mostly a shallow *reportage* of *topoi*; it lacks explanation or description of social context and interpretation of society prompted and

aided by sermon social themes. The sermons tell us the preachers' views regarding the church hierarchy, kingship, lawyers, merchants, soldiers, the rich, the poor, Jews, Muslims, women in general, marriage, widows, prostitutes, and female piety. But Taylor often seems unaware that many of the preachers' ideas are long-standing in medieval society, and she almost always ignores the actual world experienced by the preachers' audiences, which gave their sermons meaning and resonance. In chapters 8 and 9 Taylor takes up a tradition of summarizing or excerpting sermons for their topics of interest and color, thus providing source material for social and cultural historians. As it stands, however, this portion of the book is disturbingly ahistorical, especially in light of much recent study of medieval preaching and sermons that is firmly rooted in analysis of contemporary social, political, and economic experience.

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JOSEPH BERGIN and LAURENCE BROCKLISS, editors. *Richelieu and His Age*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xvii, 288. \$72.00.

This collection edited by Joseph Bergin and Laurence Brockliss, based on papers contributed to a conference held at Oxford in 1991, appears in time to commemorate the 350th anniversary of Cardinal Richelieu's death. Many of the essays published in France for the anniversary of Richelieu's 400th birthday in 1985 focused on the cardinal's distinctive attributes; the title of the present selection, however, seems to suggest a Richelieu who was more representative of his milieu than a shaper of it, more conservative than innovative. Yet given this thematic tendency, there is considerable diversity among the eight papers. Editorial "demands" and "suggestions" (p. vii) must have helped to produce a book that is a valuable addition to the study of early modern Europe and displays the present state of scholarship in a number of fields of inquiry into the regime of Louis XIII and Richelieu.

A. Lloyd Moote's lead article places Richelieu in the context of a role common to many early modern European courts, that of political favorite. Moote reaffirms the view that the "political culture and social drives of the age" counted for more than the "peculiarities of the individual favorite's status, career path, or personality" (p. 38). Nevertheless, to explain the distinctive interaction pattern between Richelieu and the king, Moote evokes the king's "strong general notions," his "sense of the characteristics he wanted in his advisers" (p. 24), even the cardinal's "special magic" (p. 20), as well as his ability to meet the "political and psychological needs" of his master (p. 24).

Hermann Weber's chapter, "Une Bonne Paix," is

the only contribution beside Moote's from a non-Anglo-Irish author. It is the first publication in English by this distinguished Continental scholar of foreign policy under Louis XIII. Students will be stimulated by Weber's view of Richelieu's war aims as designed to bring a non-hegemonic order to European Christendom and by his many citations of recent literature by scholars east of the Rhine.

Robin Briggs's essay on Richelieu and reform of the French state apparatus takes account of the point stressed by Moote (to which Weber makes no concessions) that understanding the policy aims of this dual regime depends on discerning how Richelieu and Louis XIII interacted. Briggs reviews major political issues of the reign, drawing widely on newer studies for evidence on the tension between public-spirited desire to reform the administration and constraints imposed by institutional structures and requirements for political survival.

Richard Bonney's essay, "Louis XIII, Richelieu, and the Royal Finances," lacks contextual mooring and hence focus and meaning. Charts and graphs range across previous or subsequent reigns—in one case most of the eighteenth century (p. 119)—and referents in the text dart back and forth between regimes and countries without any discernible purpose. Types of taxes under Louis XIII are named but not described (p. 101). Minute differences in sources of revenue are tabulated but clues are not given to clarify the significance of the elaborate comparisons (p. 103). One graph shows proportions of different types of expenditure by decade for the seventeenth century (p. 105), but since an important category, "extraordinary expenditures," varies radically and does not, like the others, designate an object, the graph is redrawn excluding the category (p. 107). The tree-farm remains after this pruning; the real forest does not come into view.

David Parrott's chapter, "Richelieu, the *Grands*, and the French Army," supplies a more coherent picture of how trends in financial policy shaped and were shaped by the demands of war. Developing a secondary theme from his dissertation, "The Administration of the French Army during the Ministry of Cardinal Richelieu" (1985), Parrott shows Richelieu first consolidating crown influence over army administration by removing certain inimical *grands* from their military fiefs. But, as the need for war victories became more urgent after 1635, "traditional" dynastic affiliations became less significant. Even the most loyal lords were unable to come up with the ever-mounting resources demanded of them. Military failure elicited more and more arbitrary retribution from a regime made helpless by a financial situation that "allocated an ever-increasing proportion of the crown's revenues to the servicing of loans contracted at high rates of interest" (pp. 172–73).

Edric Caldicott's essay, "Richelieu and the Arts," manages, in the scope of thirty pages, to synthesize a good deal of evidence on Richelieu's role in art and

literature. Caldicott is most informative in tracing the design of various galleries constructed at the cardinal's instigation and in describing the paintings, statuary, and ornamentation with which they were furnished. An overview of the cardinal's sponsorship of the theater throws new light on the form his enthusiasm took, although his dramaturgic tastes had "positively no impact on the cultural life of France" (p. 232). Caldicott touches little on the personal sources of these tastes (except for the model set by the self-glorifying patronage of Marie de Medici), and still less on how they may have been affected by Louis XIII's tastes. He emphasizes the "ideological gallo-centricity" of Richelieu's theatrical productions (p. 233), an aspect of his cooptation of all branches of the arts to express "the maturing concept of total politics" (p. 228). The consequence was a "new assertive French presence" in the arts, still manifest in the quest of present-day French governments for *grandeur*.

Laurence Brockliss adds significantly to his indispensable study, *French Higher Education in the Seventeenth and Eighteenth Centuries* (1987), with "Richelieu, Education, and the State." If he is mistaken in believing Richelieu was "intended originally for a military career" (p. 240), and little interested in other such motive-related matters, he deftly characterizes and documents well the balancing Richelieu managed to sustain among the claims of the University of Paris, the pressures for extending education exerted by the burgeoning *collèges*, Jesuit and otherwise, and his own discriminating educational aims for public, nobility, and clergy.

Joseph Bergin's worthy addition to the contribution he has already made in several monographs on Richelieu and his times is his essay "Richelieu and His Bishops?" Here he traces previously undocumented fluctuations in the attrition rate of bishops during Richelieu's incumbency and identifies significant changes in the median age, career experience, and education of their replacements. He constructs a subtle and convincing picture of Louis's and Richelieu's habitual division of labor and interaction in decision making that helps clarify the nature of their reformist aims for the clergy. It would have been well if Bergin could have given some account of the influence exerted on episcopal politics by the crown's ever-growing need to wring money from the clergy, but this complex subject deserves a volume for itself.

The editors should be satisfied with the quality of this product of their collaborative effort, but it is somewhat surprising to learn that the conference "brought together virtually all the scholars in the British Isles currently working on the history of sixteenth- and seventeenth-century France" (p. vii), since there are no women among the contributors to this volume. The French quatercentennial conferences resulted in several anthologies of work by men and women of varying nationalities and specializations in early modern European studies. Perhaps the

gender-distribution of historians of France is far more skewed in Britain than elsewhere.

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ELIZABETH RAPLEY. *The Dévotes: Women and Church in Seventeenth-Century France*. (Studies in the History of Religion, number 4.) Buffalo: McGill-Queen's University Press. 1990. Pp. viii, 282. \$34.95.

Winner of the David Pinkney Prize, which is awarded by the Society for French Historical Studies each year for a book written on any era of French history, Elizabeth Rapley's book provides a good example of how the imposition of outworn categories such as "Reformation" and "Counter Reformation" obfuscates important historical issues in early modern France and how careful attention to women's endeavors reveals historical change in church and society. This book charts the revolutionary course taken by devout women who translated religious inspiration into social reform. Ostensibly beholden to religious communities under ecclesiastical rule, these women were actually architects of new congregations with guidelines promoting activist social service. Focusing on varied female groups, such as the Visitation Order, the Ursulines, and the *filles séculières* in the 1600s, Rapley revamps our notions about their professional endeavors and the public context for them. She recounts the secular social pressures in France (not the religious reforms of the Council of Trent) that gave impetus to new orders for women; notes the refusal of the *dévotés* to adopt the misogynistic tradition embedded in monastic life (cloister, perpetual vows, obedience, silence); explains their voluntary adoption of a single (rather than marital) state in order to facilitate vocations undertaken not in isolation but in public; and points out the valuable feminine techniques of pedagogy and administration that emerged from work experience in teaching girls, rendering hospital care for the sick, sheltering orphans, and aiding the poor.

Rapley places the *dévotés'* bold and creative spiritual and social responses to contemporary problems squarely within the framework of rapid social change during the 1600s: the public demands for teaching and social work, which meshed with their intentions to render spiritual life a facet of public life. She also chronicles the hostility that accompanied the break of the *dévotés* from both ecclesiastical traditions and familial roles. Despite adherence to simple vows, modest dress, chaste demeanor, and propriety of behavior, the women sometimes were maligned and molested on the streets by ecclesiastical and secular officials and by ordinary people. Despite notable successes in teaching the catechism and other lessons, including reading, they were accused of usurping (inadequately) the role of priests (adequate or inadequate). Despite their zeal for a materially simple life carried on in pious congregations, their presence in

society (uncloistered) and their continued legal attachment (undowried) to family estates threatened the intent of family law on marriage and inheritance as well as the ordinary church expectations of bequests to regular parish churches and religious orders.

The face of hostility was not masked, but the *dévotés* through sheer persistence moved beyond the bulwarks erected before them; they also gained support for their innovative projects from churchmen such as François de Sales and Vincent de Paul, who were caught up in the spirit of changing times as well. Once the *dévotés* won the official, albeit grudging, acceptance of church and society for their spiritual and social renovations, some positive changes in the status of religious women followed. By exploiting the largely uninhabited, certainly undefined, space between ecclesiastical world and lay world, these women ultimately claimed and won a significantly enlarged place in both church and society.

Rapley's study brings all these events back to the fore through meticulous scholarship, careful attention to women's voices, judicious interpretation, and fine writing. Although this study concentrates on the positive side of the women's congregations and their voluntary work—teaching and charity—and hence does not detail the negative side—the involuntary detainment of women boarded on some convents by irate fathers and husbands for familial and legal reasons—the latter topic actually requires a different study all the better undertaken on the firm foundations established by this impressive book.

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ELIZABETH L. EISENSTEIN. *Grub Street Abroad: Aspects of the French Cosmopolitan Press from the Age of Louis XIV to the French Revolution*. (Lyell Lectures, 1989–1990.) New York: Clarendon Press of Oxford University Press. 1992. Pp. 172. \$35.00.

In the much acclaimed *Histoire de l'édition française* (1983), the volume incorporating the period 1660–1780 devoted nearly 20 percent of its text to French-language publishing outside France. For readers of the *Histoire* it remained an open question whether such comprehensive coverage bespoke editorial recognition of the Enlightenment's cosmopolitanism or served to advertise Gallic cultural imperialism. Elizabeth L. Eisenstein is crystal clear about where she stands in this book, drawing together the Lyell Lectures she delivered at Oxford in 1989–90. Eisenstein suggests that, as early as the 1680s, a Francophone cosmopolis, challenging Versailles/Paris-based conventions of absolutism and academism, flourished within the "fertile crescent" arching round France between Amsterdam and Avignon. Moreover, Eisenstein detects lines of cultural continuity extending from the erudite diaspora journalism of Pierre Bayle and Jean Le Clerc to the thriving extraterritorial

publishing enterprises of Liège, Bouillon, and Neuchâtel a century later.

There were good reasons for the "centrality of the marginal" in Western European cultural life during the Enlightenment. Economic considerations motivated princes and urban governments in the Holy Roman empire, the Low Countries, and Swiss cantons to relax censorship restrictions on Francophone publisher-booksellers and journalists. Meanwhile, eighteenth-century French ministries had reasons of their own for establishing levels of tacit tolerance over the importation of books, periodicals, and pamphlets. Furthermore, from Moscow to Dublin there existed a considerable market of French readers.

Eisenstein is less interested, however, in bookselling strategies or the economic and political underpinnings of literary production than she is with case studies of the producers themselves, particularly as they illustrate intellectual trends and cultural proclivities. Henry Desbordes, Bayle, Le Clerc, and Prosper Marchand are her prototypes, and she takes to task those historians who have rejected her humanistic vision of cosmopolis to emphasize grosser motives. Nevertheless, to my mind the rationale for mid-Enlightenment extraterritorial publishing did evolve dramatically from the previous generation. Even while admitting with Eisenstein that Robert Darnton overargues the case for late-eighteenth-century publishing as a pure exemplar of "booty capitalism" (pp. 23–28), any reading of the correspondence of Marc-Michel Rey, Pierre Rousseau, or S.-F. Ostervald will reveal a balance of concerns quite different from those of Desbordes or Marchand. By the 1760s, markets were larger, competition keener, piracies more flagrant, costs higher, investments more risky. Like it or not, cosmopolis had become big business.

This book is not the product of archival research, and it draws heavily on the work of others. To her credit, Eisenstein acknowledges this generously; yet at the same time she is a critical reader. She urges historians of the book and of journalism to cease viewing their disciplines as discrete entities and chides Roger Chartier for devaluing intellectual history in his work on the "cultural uses of print." She considers William Sewall's reading of the *Encyclopédie's* plates on work processes ahistorical and questions Margaret Jacob's use of Masonic glue as the binding force among extraterritorial publishers. Yet Eisenstein finds that these publishers' houses represent more appropriate forums for advanced ideas than the Paris salons emphasized by Jürgen Habermas and Dena Goodman. Among contemporary historians, it is Darnton who is on the receiving end of Eisenstein's most stunning rebuke, likely because of his significant influence. For her, Darnton's use of material ambition, career blockage, and economic motivation as determining factors in cultural history has produced a distorted interpretation of prerevolutionary cosmopolis. Although one might question Eisenstein's critique of theses nearly two decades old and subse-

quently nuanced by several papers, in her view the time appears ripe for a renewed history of ideas; its most appropriate locus is the history of early modern publishing.

RAYMOND BIRN
University of Oregon

ALAIN CABANTOUS. *Les côtes barbares: Pilleurs d'épaves et sociétés littorales en France (1680–1830)*. Paris: Fayard. 1993. Pp. 311. 145 fr.

This is a book about those coastal folk who used to plunder ships wrecked or stranded on French coasts. "Their cleverness makes me think they are accustomed to this sort of enterprise," wrote a shipwrecked American, William Lee, in the early nineteenth century when he saw the inhabitants of Soulac breaking open the barrels of provisions and tearing off the coverings with loud swearing and threats (p. 64). This situation, Alain Cabantous reflects, was as theatrical as a mutiny and was taken up by imaginative writers in that spirit (p. 258). In an effort to cast some light on wreckers and their evolution in modern times, he has used records, often in legal cases, of 109 wrecks on the French coasts. These he considers from as many different viewpoints as the sources permit: the admiralty and other courts of law, the clergy, the government, and even the neighbors or bystanders. Cabantous makes full use of the dramatic and sinister aspects of the subject, citing cases of ships lured onto the rocks with false lights by pirates who then murdered any survivors and looted the wrecked vessel. But what interests him most is the evolution of wreckers and wrecking as a social phenomenon. He is convinced that legends about wreckers persisted long after the coastal dwellers had in fact mended their ways and taken to assisting shipwrecked people whom they might once have assaulted. A new humanity is visible in the behavior of seaside communities during the Enlightenment of the eighteenth century, but imaginative writers invented or kept alive stories about seaside violence that no longer had any foundation.

The chronology of the humane improvements Cabantous discusses is distressingly vague. The narrative sometimes turns into a cumulation of surmises and rhetorical questions, but this is perhaps a sign of intellectual honesty in a difficult field of history. Lacking neither intelligence nor style, the book is not a methodical study so much as an essay embroidering on themes in the psychology and sociology of wrecking communities in times past. Cabantous's prose smacks, on some pages, of the *New Yorker* or the *Times Literary Supplement*. What emerges from it is that the French gradually ceased to victimize the survivors of shipwrecks, but went on plundering wrecks for much longer. Improvements came gradually, and what persisted the longest was a deep-seated and widespread principle that whatever the sea brought in was flot-

sam, there for the taking. As Cabantous suggests, this is almost an instinctive principle, and indeed, I recall as a boy on the Pacific coast bringing home quantities of fir two-by-fours from wrecked lumber barges as of right and almost by instinct. Universal questions about human behavior surface quite often in this thoughtful book, but only as a background for reflections on a particular form of criminal behavior at specific times in certain parts of France. Maps show where the wrecks in question took place; many, of course, on the rocky coasts of Brittany and on the busy coasts of the English Channel. This is an original essay that might appeal to many social historians.

J. F. BOSHER
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MANUELA ALBERTONE. *Moneta e politica in Francia: Dalla Cassa di sconto agli assegnati (1776–1792)*. Bologna: Mulino. 1992. Pp. 456. L. 50,000.

Manuela Albertone states in the introduction that her primary goal is to show the interactions between economic thinking and French politics and administration in the later eighteenth century, culminating in the debates over the debt crisis in 1787–91. Her book ranges widely through the economic theory and practice not only of France but also of Great Britain, the United Provinces, and the new American republic, all three of which served sometimes as models, sometimes as warnings for those who attempted to create a coherent economic policy for revolutionary France.

For historians of the French Revolution, the book's primary interest will be in the first part, on "Credit, Politics, and Society," and in the fourth and final section that places the emergence of the fiscal experiment of the *assignats* in the context of earlier developments. Rather oddly, the book breaks off abruptly in 1792 after tracing the objections of leading critics of the paper money policy such as Marie-Jean Condorcet and Pierre Dupont de Nemours. The second and third sections are more familiar, consisting essentially of a fairly conventional discussion of the history of eighteenth-century economic ideas, first in France and then in Great Britain.

Albertone's research in primary materials is admirably thorough, including not only the vast array of pamphlets, broadsides, and treatises in the Bibliothèque nationale but also a number of manuscripts from the Archives nationales and provincial repositories. Sometimes the author is unselective, devoting considerable space to deservedly forgotten publicists without fitting their ideas into the primary intellectual developments of the period, but her analyses of their work are always clear and thorough.

The book certainly reinforces the impression of the enormous role that foreigners played in France's financial crisis. The Swiss figure most prominently—Jacques Necker, Isaac Panchaud, Étienne Claviere,

even Jean-Paul Marat—but there are also Prussians, Dutch, English, Scots, Irish, and a scattering of Americans. Clearly, the "foreign plot" that became a Jacobin obsession during the Terror had deep prerevolutionary roots.

Albertone's central thesis, as set forth in the first chapter, is hard to dispute in this era of postrevisionist historiography. The corporate structure of society, the pervasive influence of venality within the administration, and the debased but persistent theories of royal absolutism all mandated against genuine reform of France's finances. Only in 1789, with the creation of revolutionary politics and the project for the creation of an entirely different kind of state, could another kind of solution appear. Because that solution entailed turning the royal debt into the national debt, the seizure of the lands of the Roman Catholic church, and the issuing of paper money whose financial backing was to be those same lands, it raised serious new problems that no revolutionary regime was able to resolve. But that is another story.

CLARKE GARRETT
Dickinson College

SYLVIA LAVIN. *Quatremère de Quincy and the Invention of a Modern Language of Architecture*. Cambridge, Mass.: MIT Press. 1992. Pp. xvi, 334. \$39.95.

Quatremère de Quincy has long been portrayed in the literature as a bastion of conservatism, stalwartly upholding a late-eighteenth-century academic classical ideal well past its time. In this remarkably clear, well-written book, we are given another view of the man, before 1816 when he became Secrétaire Perpétuel de l'Académie des Beaux Arts and emblematic of an entrenched neoclassicism. Born in Paris in 1755, Quatremère studied law and then sculpture before turning to architecture after a trip to Italy in 1779; he in fact built little and as an architect is known mainly for his work in transforming Soufflot's Church of Ste Geneviève into the Panthéon in the 1790s, his dictatorial control over the granting of government commissions as Secrétaire of the Académie, and his role in the debate over polychromy in classical antiquity in the 1830s. Sylvia Lavin's book discusses none of this, instead focusing on his lesser-known earlier theoretical essays, analyzing them in relation to the writings of others and placing them in the intellectual context of their time.

The book is not about architectural forms, or what was actually designed or built, but rather the ideas, attitudes, and values that helped to shape or influence architectural practice in France in the post-revolutionary years. The book is a revision of Lavin's dissertation, supervised by Robin Middleton, one of the leading scholars of neoclassical and nineteenth-century architecture. It reflects the current interest in history and theory in architecture schools today.

Lavin examines Quatremère's theoretical work, in

particular his essay on Egyptian architecture of 1785, written at a time when the Greco-Roman ideal was being challenged by new architectural forms that were expanding the range of formal vocabulary. Written on the eve of major archaeological discoveries in Egypt, then revised for publication twenty years later, the essay was a reaffirmation of Greek superiority, upholding it as an architectural ideal and a model for contemporary architectural practice. Departing from prevailing views on the origin and development of architecture, Quatremère postulated a theory of types, arguing that Greek architecture was superior not simply because it was the most beautiful or the simplest and purest but because, having developed rationally from one of several generic types, it was capable of a wide range of formal expression that other architectures lacked.

Rather than changing through a natural or divinely preordained evolutionary process, architecture like language developed through social convention, shaped and determined by rational, conscious, human-made, and socially conditioned choices. From a series of basic universal types or solutions (of which Quatremère identified three: the cave, the tent, and the hut), and responding to different modes of living, various architectures emerged from different social, climactic, and historical conditions. Not all developments were equally meritorious, however, and the classical, developing from the primitive wooden hut that offered both solidity and flexibility lacking in the other two types, he saw as exhibiting the most expressive potential, hence offering the best model for modern society.

In his comparison of Egyptian to Greek architecture, Quatremère acknowledged the legitimacy of both; he endorsed classicism not as an abstract formal ideal, but as a way of conceptualizing architecture as a socially and historically contingent elocutionary art. His essay is important both in understanding the conceptual framework of architecture in the late eighteenth century and in understanding how and why Quatremère, for all his lack of architectural experience (and perhaps talent), happened to be appointed secretary of the Académie, a position of considerable power. More importantly, however, according to Lavin, is his more subtle lasting legacy to the history of architecture: his theory of types and conception of architecture as a language, socially determined and hence culture-bound and capable of responding to social change.

The subject is dense, complex, and highly specific; Lavin writes about it with admirable lucidity. Her work should be of particular interest to those wanting to know more about the conceptual framework of architecture at the time—what the prevailing values were and how they changed—and to those in the architectural profession interested in the theoretical roots of architecture, particularly the notion of archi-

tecture as a language conveying specific, culturally determined meaning.

MEREDITH L. CLAUSEN
University of Washington

JEAN-PIERRE HIRSCH. *Les deux rêves du commerce: Entreprise et institution dans la région lilloise, 1780–1860*. (Civilisations et Sociétés, number 82.) Paris: École des Hautes Études en Sciences Sociales. 1991. Pp. 534. 380 fr.

Jean-Pierre Hirsch's *thèse d'état* looks at first glance like a representative sample of that well-known genre, the glory or the bane (depending on whom one consults) of the French scholarly tradition. Its heft recalls the masterpieces of Georges Lefebvre (on this same time and region) or Emmanuel Le Roy Ladurie: 500 pages of small print, replete with multipage tables, complex charts and graphs that are difficult to decipher, and a weighty bibliographical apparatus at the end. Hirsch's work is unusual, however, in its methods, subject matter, and organization. The massive array of evidence is marshaled in support of an original, sustained line of argument from which he never strays too far. The nature of that argument determines both the opening and closing dates of the study, which significantly cross the revolutionary divide, and the combination of methods, employing the approaches of business history, analysis of family fortunes, and detailed examination of legislative reform and government institutions.

Hirsch's argument can be easily stated. The French Revolution, long believed to have inaugurated the regime of *laissez faire*, brought far fewer changes to the business practices and to the way of life of the French commercial elite than is normally supposed. Of course, it has long been argued that French merchants and industrialists in the nineteenth century did not embrace the reign of freedom of contract with the same alacrity as their colleagues across the Channel. But Hirsch wishes to go further. He contends that a free competitive regime is an impossible ideal and that the merchants and entrepreneurs of the Lille region, however enamored of that ideal they may have become, could not help but draw back from it in practice. Their own mutual relationships, their relations with employees, their dependence on state regulation and state judicial institutions, when viewed together, added up to a regime that was not corporate in the Old Regime sense of the term, but something that did not at all match the rhetoric of free enterprise that dominated both public and private discourse about postrevolutionary economic life.

Hirsch's demonstration proceeds in clear stages. He shows, first of all, that—rhetoric to the contrary—the nineteenth-century boom towns of the Lille region were not the peculiar preserve of new self-made men. Rates of entry of new family lines into the business elite, he demonstrates, were at least as great

in the eighteenth century as during the classic phase of mechanization that began in the 1830s, during which no more than a third of Lille-area entrepreneurs succeeded without the aid of solid backgrounds in local commercial families. Second, in spite of the drastic tone of the *loi d'Allard* and the *loi Le Chapelier* of 1791, which abolished the guilds and outlawed all constraints on free trade, Hirsch shows that an impressive array of Old Regime regulations on trade were taken over almost unchanged by subsequent legislation, all of it piecemeal and unsystematic, but adding up to serious limitations on freedom of enterprise. Third, Hirsch shows that the networks of close ties by which eighteenth-century merchant families sustained their credit and the faith of their customers were little different from those which nineteenth-century captains of industry used to gather the resources for bold ventures in mechanization. Within such networks, Hirsch argues, price could not function as a clearing mechanism in the classic sense because relations were "personalized." Reputation counted for far more in deciding whom to lend money to than did the anticipated rate of return. Fourth, Hirsch notes that protecting the delicate tendrils of credit against sudden crises of faith was a matter of such public importance that commercial law and *tribunaux de commerce* were widely accepted as a necessity, even though their operations caused constant uneasiness, and even though they amounted to a glaring (if seldom mentioned) exception to the fundamental principle of equality before the law. In effect, persons subject to the jurisdiction of commercial law were treated differently from others.

Having laid out the basic structure of his argument, Hirsch then explores the gradual unfolding of commercial life from the Old Regime through the Industrial Revolution, moving from one detailed case study to another. Based in family ties and marital alliances, the Lille-area commercial community sought, through changing legal and administrative structures brought by successive crises, to find a happy blend of commercial adventure and institutional security. These are the "two dreams" of Hirsch's title, a dream of rapid and successful enrichment in the context of free trade, on the one hand, and a dream of a stable management of family enterprise, protected by strict regulation and custom, on the other. The two were formally incompatible; yet they can be seen guiding action through reforms of credit institutions, the establishment of new regulations governing workers, and the development of the steam-powered textile mills.

In a final section Hirsch raises the question whether Lille, in the French context, or France, in international perspective, were really so different. Clearly he believes similar arguments could be made about the nature of business practices and state intervention in both England and the United States. One would hope that subsequent researchers will take up the challenge. It is a pleasure to appreciate a powerful mind

bringing together such a remarkable array of evidence in the service of a single, clear thesis, evidence whose implications Hirsch teases out always with a genuine sympathy for the people whose history he has so boldly rewritten. Although excellent business history, Hirsch's work finds its place, as well, within the tradition of historiography stretching back to Karl Polanyi's argument, in *The Great Transformation* (1944), that a true market system was a destructive and socially impossible arrangement whose dismantling began almost as soon as it was instituted, or to E. P. Thompson's notion of a "moral economy," counter-ideal to the competitive market that early nineteenth-century artisans clung to with desperation (*The Making of the English Working Class* [1963]). Hirsch's work, in effect, provides a guide to the moral economy of a business elite and points toward the complex and constantly shifting political arrangements (as opposed to mere market pressures) that made industrialization possible.

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MAURICE CROSLAND. *Science under Control: The French Academy of Sciences, 1795-1914*. New York: Cambridge University Press. 1992. Pp. xvii, 454. \$120.00.

Maurice Crosland's book is a carefully crafted institutional history of the French Academy of Sciences in the nineteenth century; based on the relevant archives and serial publications, it can be expected to stand as definitive. During the revolutionary and Napoleonic periods, the old royal Academy of Sciences was transformed into the First Class of the Institute of France, although it was still loosely called the Academy of Sciences. Crosland's title suggests an interest in the control of science, but his generally benign view of that control extends well beyond government management of the Academy, focusing on the Academy's de facto definition and encouragement of an "official science."

French science, as Crosland points out, uniquely combined a high degree of centralization with relatively early provision for specialization. By 1803 the First Class of the Institute encompassed eleven sections of (almost invariably) six elected members each. The Academy met as a single body; attendance was encouraged in part by minor adjustments in the members' remarkably modest government stipends. Yet the establishment of sections for named subfields made room for a degree of specialization that was unusual, at least for the time. In a definitive break with the British tradition of natural philosophy, Crosland shows, French science in effect created the fields of experimental physics, geology, and zoology (pp. 33-39).

The main institutional functions of the Academy could be roughly grouped under the four headings of registration, evaluation, representation, and patron-

age. At its regular meetings and, from the mid-1830s on, in its renowned *Comptes rendus*, the Academy registered new scientific work, whether reported by one of its members (in his or her own behalf or for a client) or sent to it from all over France and Europe. It could take note of communications without commenting, but it could also issue evaluative reports, acting through one or more of its expert members. Although valuable in itself, a favorable report could lead to publication in the *Comptes rendus*, or in the older and more comprehensive *Mémoires*. Beyond that, the Academy issued a growing number of small awards and substantial prizes, which eventually came to function as research grants as well. Finally, through its elections, the Academy dramatized the eminence that could be achieved through scientific work. Regular membership required residence in Paris, but even corresponding members (and a tiny elite of associate members) from the provinces and abroad could feel that they belonged to a distinguished and meritocratic elite. The Academy's right to nominate candidates for positions at major educational institutions (not including the Faculties of Sciences) rounded out a decidedly hierarchic and increasingly gerontocratic structure that symbolized the high prestige of official science while encouraging the emergence of a network of patron-client relationships.

There were few cases of direct government interference in the affairs of the Academy; the most serious occurred under the Restoration. Most Academicians avoided political entanglements and adjusted easily to changing regimes. (This pattern is not abnormal.) Several scientific discussions were affected by extrascientific ideologies. Thus, religious commitments played a role in the reception of evolutionary theories, as in the controversy over transformism and the Louis Pasteur-Félix Pouchet debate over spontaneous generation. Around 1900 antimaterialists encouraged Gustave LeBon's spurious claims about black light (which the Academy never backed) as well as René Blondlot's report of N-rays (which had some official support). Crosland comments that the outcomes of scientific research may be "influenced" but never "determined" by sociopolitical "pressures" (p. 429). Stated that broadly, his thesis is unlikely to draw criticism.

In any case, Crosland's real question is to what extent the organization of science in France affected the rate of scientific advance (rather than outcomes). Although his assessment of French science is generally positive, he also notes ominous symptoms that worsened toward the end of his period. Limited in its membership and in the designations of its sections, the Academy aged while being outflanked by more specialized disciplinary associations and journals. The virtual exclusion of provincial scientists, the rapid expansion of the Faculties of Sciences from 1880 on, and the partly numerical superiority of German university science by 1900 are all symptoms that raise a question which Crosland might have addressed even

more explicitly: Was the mixture of centralization and specialization that aided French science for a time ultimately unable to compete with less hierarchic and more multicentric institutional structures?

Fritz Ringer

University of Pittsburgh

MICHEL LAGRÉE. *Religion et cultures en Bretagne (1850-1950)*. Paris: Fayard. 1992. Pp. 601. 180 fr.

One of the most powerful presumptions of modern historiography is that religion is more acted on than determinatively influential in the evolution of contemporary society. Scholarly specialists in contemporary religious history nonetheless typically approach their subject from one of two distinctive points of view. Some focus narrowly on religions in and of themselves, studying them apart from all other human activities. Others, the larger number and more theoretically ambitious in their analytical perspective, seek to put religion in a broader social context, seeing it as but one of several crucial elements of exchange and alteration in the constantly fluctuating life of contemporary society.

It is this second and broader approach that Michel Lagrée takes to his study of religion and culture in Brittany during the century after 1850. The main axis of his interpretive stance is the growing secularization of Breton society, especially in the twentieth century as religious practice dipped below 15 percent in many areas. As elsewhere in Catholic Europe, a traditional, backward-looking, and largely rural civilization on the Armorican peninsula progressively yielded its place to a modern, future-oriented, and predominantly urban culture. The result has been the transformation of Brittany into what Lagrée aptly calls a kind of French Denmark whose powerful economic base is a complex of dynamic agricultural industries and food-processing firms.

What has been particularly striking about this process is how Brittany's steady march to modernity since the beginning of this century has been characterized by a local clergy anxious to adapt to the times and borrow from modern civilization those things that might allow Catholicism to survive and flourish on the peninsula. Thus, it was far-seeing Breton priests, religious organizations, and practicing lay Catholics who became the most active champions of the region's agricultural modernization and industrial development. At the same time, in a self-conscious effort to maintain the church's influence among an increasingly disaffected youth, Breton parish clergy have been able to accommodate themselves to modern mass culture and successfully use the schools, sports, film, and even the mass-circulation press to modernize and keep relevant for the local population the spiritual claims of the Catholic church. In the process, as Lagrée persuasively shows, the Breton clergy has assured for itself a significant part of the

management of Brittany's overall modernization. Indeed, he wonders in his conclusion if the Breton experience is not part of a greater European-wide phenomenon that has seen regions long marked by Catholic traditionalism—Bavaria, Flanders, and Venetia to name three—emerge as among the most socially, economically, and culturally effervescent areas of the Continent during the second half of the twentieth century.

Unfortunately, it is impossible in this small space to do full justice to Lagrée's intelligently argued and data-rich volume on the intimate relationship between religious life and social and cultural change in modern Brittany. His long discussion in the first part of the book of the region's deeply imprinted Catholic heritage in the nineteenth century is superb. Almost as impressive is his much briefer discussion in chapter 9 of the close interconnections between internal and external migratory patterns of Bretons, the steady loss of their religious faith, and the precipitate decline of their ancestral Celtic language during the contemporary period. The book concludes with sixty-seven maps and other graphic illustrations that effectively summarize the rich social, economic, and cultural data that Lagrée has been able to cull from over 20,000 numbers of episcopal, diocesan, and parish publications in Brittany that form the primary evidential base for his analysis.

JACK E. REECE
University of Pennsylvania

MARIE-CLAUDE GENET-DELACROIX. *Art et état sous la III^e République: Le système des Beaux-Arts 1870–1940*. (Histoire de la France aux XIX^e et XX^e siècles, number 31.) Paris: Publications de la Sorbonne. 1992. Pp. lviii, 433. 190 fr.

By "système des beaux-arts," a term borrowed from the political philosopher Alain, Marie-Claude Genet-Delacroix means "an integrated system of perception, organization, administration, and preservation," linked to the totality of a cultural milieu and notably to its educational structure (p. 7). She argues here that the French Third Republic created this system in order to rationalize and make socially useful practices of cultural patronage it inherited from prior monarchical regimes. For Genet-Delacroix, the structures established in the art world paralleled a larger system of universal and progressive values; in this way the fine arts played an important role in the rooting of democratic values in France.

Although Genet-Delacroix asserts that an analysis of the relations between the state and the arts can only operate in terms of discourse, she devotes most of her attention to the administrative dimensions of that relationship. The first three substantive chapters, amounting to nearly half the book, examine the Conseil supérieur des Beaux-Arts, or Higher Council of Fine Arts, which constituted in the author's view

the "pivot" of the system (p. 4). A consultative body established in 1875 to advise the government on matters of cultural policy, the council was composed of ex officio members from various state artistic institutions, as well as artists, critics, and politicians. Through the 1880s the council seems to have had some real influence on the shaping of policy, notably in the development of a general art curriculum at every educational level. But as it ballooned in size—from thirty-two in 1875, its membership grew to forty-five in 1880, fifty-five in 1887, eighty in 1905, and ninety-one in 1909—the council became increasingly honorific and smaller, and more specialized advisory boards took over its real functions.

The second half of the book deals with the government's cultural apparatus as a whole. Only briefly (during Léon Gambetta's short-lived Grand Ministère of 1881–82) an independent ministry, for the rest of the Third Republic the Fine Arts Administration was attached to the Ministry of Education, although it had its own budget and, often, the political leadership of an undersecretary. Through a complicated series of bureaucratic combinations and divisions, the Fine Arts Administration came to control such diverse domains as theater subsidies, public buildings, the Paris museums, and the national porcelain and tapestry workshops. The various parts did not always form a coherent whole, and bureaucratic rivalries, as well as fairly constant political sniping, claimed a great deal of the administration's time and attention, particularly at the highest levels. Genet-Delacroix argues, however, that consistent policy goals informed the work of the administration, although these shifted from a predominantly pedagogical orientation in the first decades of the republic to a greater concern with social action, with making culture available to a broader public, after World War I.

This book originated as part of the author's *thèse de doctorat d'état*, and it shows all the hallmarks of the genre: massive documentation, including a bewildering array of tables and charts, an almost obsessive closeness to the sources, and an elaborate organization without any apparent rationale beyond its own meticulousness. The thoroughness of Genet-Delacroix's research in the area of arts administration is impressive, and she provides much information, both in her text and in the extensive documentary appendixes, that students of French cultural policy will find useful. On an analytical level, the book confirms recent scholarship on the ways in which republican ideology became inscribed in preexisting and emergent practices of official patronage. Too many of Genet-Delacroix's broader claims, however, depend on assertion rather than argument, and ultimately the book's compass is too narrow to support the author's view of the centrality of the republican artistic "system" in transforming mentalities and cultural practices in modern France.

DANIEL J. SHERMAN
Rice University

REINHOLD BRENDER. *Kollaboration in Frankreich im Zweiten Weltkrieg: Marcel Déat und das Rassemblement national populaire*. (Studien zur Zeitgeschichte, number 38.) Munich: R. Oldenbourg. 1992. Pp. 338.

Reinhold Brender's work is the most thorough historical study yet to appear on the career of Marcel Déat and the Rassemblement National Populaire (RNP), vocal proponents of pro-Axis collaboration during the German occupation of France in World War II. Brender consulted extensive French and the German governmental documentation for the wartime years, much of it requiring special permission to see. His analysis of the composition of the RNP is based largely on regional studies of the pro-German parties by researchers for the Comité d'histoire de la deuxième guerre mondiale during the 1970s, which unfortunately are incomplete and omit Paris, the center of collaborationist strength. Not all the relevant materials in the French national archives were made available to Brender. The wartime collection in the Paris police prefecture archives was also closed to him, and the main body of RNP party records was destroyed after the Normandy landings (pp. 21, n. 28; 183). Many of his conclusions are, accordingly, tentative.

Lacking a morphology of collaboration, Brender appears to accept Jean-Paul Sartre's simplistic definition of it as "the expression of disintegration" (p. 301). Judgments of collaboration, however, are made only in historical context and they may change over time. Examples include the shifting attitudes in France toward the work of Louis-Ferdinand Céline and Paul Morand and the more recent Stasi revelations occasioning historical revision in Brender's own Germany. Déat's collaboration, which he himself characterized as "European" (p. 249), foreshadowed to some the European Community. Brender also considers Déat and the RNP to have been fascist, at least after 1942, but his evaluation would be strengthened with a historical perspective wider than reference almost exclusively to the German case. The World War I front experience (p. 273) was important in Italy and Germany but not in Spain or Portugal. Identifying fascism with totalitarianism (p. 275) is at variance with the Italian experience. Lacking a comprehensive framework for collaboration or fascism, Brender links the latter with a death fixation. He notes that in 1943 Déat called for a new Europe to arise "in effort and in pain, in war, sweat, sacrifice, tears, and blood" (p. 294), but forgets that it was wartime and that Churchill had evoked similar sentiments in 1940.

Brender does well in balancing the continuities and shifts in the often vague political ideas of Déat, who moved from socialism in the early 1930s to National Socialism during the occupation. He notes that Déat was one of the few fascists who referred to the French Revolution, seeing the single party as a latter-day Jacobin club in 1943 (p. 299). Here, again, compari-

son is instructive. Representing Italy at an international congress of philosophers in 1934, Ugo Redano also argued that fascism was the perfection of the community's political will as conceived by Rousseau and practiced during the French Revolution (in "La crisi della democrazia e le nuove concezioni politico-sociali," *Attes du huitième congrès international de philosophie à Prague 2-7 septembre 1934* [1936], 610, 616).

Déat's RNP was one of some half-dozen bitterly competing pro-Axis parties in occupied France. Brender notes that its membership was older, more female, and marginally more attached to Republican legality than those of its rivals among the other would-be single parties of the day. Déat, who, a day before the German invasion of the Soviet Union, predicted (in *L'Œuvre*, June 21, 1941) that these two powers would not fight each other, was consistently wrong on the course of the war, as Brender shows. Germany would not attack France, Déat wrote in mid-April 1940, during the *drôle de guerre* (p. 78), a few weeks before the onslaught in the west. In February 1945, after the liberation, he still believed that Germany would win the war (pp. 251-52). The Allied victory of 1945 ensured that Déat would be condemned, along with Marshal Pétain, Pierre Laval, and Vidkun Quisling, as collaborators, but rehabilitations such as that of Father Tiso in Slovakia should remind us of the temporal nature of such judgments. Brender's worthy contribution on Déat and the RNP will help make such judgments more informed.

BERTRAM M. GORDON
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SARA T. NALLE. *God in La Mancha: Religious Reform and the People of Cuenca, 1500-1650*. (Studies in Historical and Political Science, 110th series.) Baltimore: Johns Hopkins University Press. 1992. Pp. xvlii, 306. \$35.00.

In recent years Spain's particularly rich archives have been attracting the attention of a growing number of early modern historians of religion and religious dissent. In this fine book Sara T. Nalle makes the best of what these archives have to offer and presents a persuasive and nuanced account of Tridentine reform in the diocese of Cuenca.

Nalle's avoidance of two methodological pitfalls adds much to the quality of this study. First, she does not rely excessively on Inquisition records and hence does not view the reform of the laity as being driven primarily by coercion from above, an interpretation to which these records can easily lend themselves. Instead, by also using diocesan, municipal, and notarial records, Nalle is able to show how the laity often espoused the cause of the reformers, with municipal councilors, for instance, complaining about absentee bishops and lax parish clergy. Second, Nalle, like William Christian, eschews posing sharp dichotomies between urban and rural or elite and popular reli-

gion. Sensitive to the links between urban center and rural hinterland and to the urban ambience of small communities, she finds that, as a result of clerical reform, the priests in villages attained an educational level nearly the same as that of their counterparts in the cities. With their guidance, poor folk came to know their basic prayers almost as well as the rich did. People of all social groups shared in a varied religious life that combined private devotion with participation in confraternities and communal cults often regarded as merely popular.

There is little that Nalle does not touch on in her analysis of the process of reform. She begins by showing that the attempts at reform in the pre-Tridentine era had only a limited impact in Cuenca. After 1545, however, efforts were more consistent and systematic: bishops used ecclesiastical courts to discipline recalcitrant and negligent clergy, while inquisitors educated and corrected the laity in matters of doctrine and sexual morality. An important consequence was that the people of Cuenca came to identify with church and monarchy in their international struggle against Protestantism. Pivotal to the success of reform was the improvement in the quality of the clergy, and Nalle demonstrates that new, largely Jesuit, colleges raised the standard of clerical education and that the number of clerics with university degrees dramatically increased. The problem of absenteeism also was rectified to a significant degree. Nalle employs Inquisition records to measure the impact of reform on the laity. Although, as she admits, those tried by the Inquisition were not necessarily typical, her conclusions as to the laity's increased religious knowledge and observance seem sound enough. Her observations on gender difference in religious practice are interesting: women attended mass and confessed more, while men purchased more indulgences.

Thorough research enables Nalle to shed light on various aspects of private and public devotion. Regarding public devotion, Nalle finds, for instance, that confraternal devotions became more standardized and consistent with those advocated by the post-Tridentine church. In the private realm, after 1545 ownership of religious books increased among members of all social classes, with the educated possessing books of hours, mystical works, and the like, and the less educated relying on broadsheets and pamphlets. Moreover, the church's teaching on purgatory affected the way in which the people of Cuenca approached death. The demand for masses to be said for souls in purgatory increased substantially, and by the seventeenth century, in part because of demographic and economic crises, the emphasis was mainly on one's own soul. Nalle views this increasing investment in such masses as excessively mechanical, a phenomenon that led to a "subversion of faith" (p. 205) by the end of the seventeenth century. Whether or not one agrees with this rather pessimistic conclusion, there is little doubt that this carefully

researched work will become one of the standards in the religious history of early modern Spain.

MARK D. MEYERSON

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ANDREW PETTEGREE, *Emden and the Dutch Revolt: Exile and the Development of Reformed Protestantism*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xii, 350. \$84.00.

The Grosse Kirche of Emden had this inscription over one of its doorways: "God's church, persecuted and driven out, has here been given succor" (Gods kerk vervolgt, verdreven, heft God hyr trost gegeven). This inscription summarizes the theme of Andrew Pettegree's study of the Reformed church at Emden, Germany. He tells the story of the Emden church, located in East Friesland, and its work of succoring the "second" Dutch Reformation and the Dutch Revolt against Spain. The book centers on the years 1554 to about 1580.

The opening chapter places Emden within the context of the Dutch exile movement during the Reformation. With repression at home, the leadership of religion—the Reformed religion, at least—shifted to churches of refugees abroad. The Dutch-German Emden church was one of these "mother churches" (chap. 3) that nourished Dutch Calvinistic Protestantism during desperate times. Other overseas exile churches were at London, Norwich, and elsewhere, and each of these played a similar role at one time or another. Pettegree calls these churches the "nurseries" of Dutch evangelical religion (p. 3). After the introduction, the author describes the foundation and inner workings of the Emden church at home (the functioning of Reformed discipline, preaching, and poor relief), and in later chapters he tells about its leadership of the fledgling Reformed movement in the Low Countries. The northern religious outpost of Emden harbored the refugees from the home country and sent leadership and resources back to the underground church. Emden was not a particularly significant town in world history, but at this unique time, because of its location and openness to religious innovations, it played a large role in Reformation affairs. The consistory records of the Emden church are very rich (now in print up to 1620), and Pettegree has made good use of these resources.

An interesting aspect of the book is the treatment of printing activities at Emden (chap. 4). The printed book was one of the chief ways of supporting the Reformation, and the Emden print shops produced a large supply of pious books, many of which were subsequently smuggled back into the Low Countries and used effectively by the Protestant movement. There was a big traffic in Bibles. The author has constructed a bibliographical appendix of 240 books printed at Emden from 1554 to 1585; this will be very useful for future reference. The printing mission of

the Emden printers was threefold: they produced Dutch books for Dutch Calvinists, English Protestant books in the reign of Queen Mary, when the Protestant door was closed in England, and also a good number of Anabaptist books. Pettegree, however, discounts the theory that the Nicolaes Biestkens Bibles, much favored by Anabaptists, were produced at Emden.

Although the main emphasis is on the Dutch Reformed churches, Pettegree also gives some attention to other Protestant movements of the Dutch Reformation, which were competing with the Calvinists, namely the Anabaptists and the Lutherans. Anabaptists were numerous in the Netherlands. They were also active in Emden, and Pettegree could possibly have gone further in noting how the Emden Anabaptists, like the Calvinists for their people, played a nurturing role to Anabaptists in the Netherlands. Churches of Emden supported the Dutch Reformation in more than one way. Emden also had small English and French Reformed churches, apparently with their own consistories, and they would have had many international religious contacts. The Marian exiles of England depended on Emden for printing. There was also a small Lutheran movement at Emden, but it did not fare well in that period. The study of Emden churches, because of their international connections, touches on events in many countries.

Pettegree's excellent book is a welcome addition to church history and to the history of religious printing and publishing in the Reformation.

KEITH L. SPRUNGER
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WYGER R. E. VELEMA. *Enlightenment and Conservatism in the Dutch Republic: The Political Thought of Elie Luzac (1721–1796)*. (Speculum Historiale, number 13.) Assen, The Netherlands: Van Gorcum. 1993. Pp. ix, 218. f. 49.50.

Elie Luzac, the hero of this book, was a true phenomenon of the international intellectual world of the second half of the eighteenth century. A scion of the Huguenot refuge in Holland, he was the representative of the most advanced thinking on subjects like freedom of the press and expression in the middle of the century; yet by the end of his life, in 1796, he would become a prolific opponent of any proposed political change. Although Luzac's contribution to legal, moral, and political thought has been touched on before, this book is the first to delve deeply into his massive printed output and come up with a rich sample of his thought.

Wyger R. E. Velema claims to be a follower of the Cambridge school, or the "new history of political thought," which "proposed to make language the central concern of the historian of political thought or—more accurately—political discourse" (p. 4). This may be a worthy goal, but there is not much evidence

of "new history" in his study of enlightenment and conservatism. What the author does is essentially what many of us have attempted, that is, to weave together from the texts a coherent, interesting, and intellectually satisfying narrative that puts those texts in their historical context; this he has done extraordinarily well in an excellent book.

Luzac was an accomplished lawyer, successful publisher, outspoken publicist, editor and annotator of Montesquieu and Christian Wolff, contributor to the *science des moeurs* and the debate over the role of natural law, economic historian of *Holland's Riches*, and a formidable political polemicist. The culmination of this last effort was a spectacular outburst of "well over six thousand pages in a ten year period," devoted to the refutation and destruction of the Patriot "assault on civilization" (p. 164).

The Dutch republic had a long history of popular uprisings in support of the Orangist and opposed to the Regent's (or state's) faction down to 1747, when William IV was restored as Stadhouder in all the provinces. At that time Luzac was an outspoken defender of a Lockean argument for the rights of the people. He was equally outspoken in defense of the broadest possible rights to publish even the most controversial views. Yet in the 1780s, as the Patriot Movement gained strength, he argued as vehemently for censorship and the restriction of popular influence on the government. For Velema this seeming reversal of principles is entirely consistent, since he sees these later developments as the logical continuation of Luzac's opposition to Rousseau (he was among the first and very few Dutchmen to recognize the pernicious implications of Rousseau's primitivism) and his increasing admiration of Wolff's "conservative vision of natural law, stressing man's natural inequality and dependence" (p. 179). Some of us might not be so generous.

Velema also takes up Luzac's violent opposition to the Patriots on the grounds that they favored popular sovereignty, that is, what Luzac considers democracy. But it is clear that the Patriots, as represented by J. D. v.d. Capellen (*To the People of the Netherlands* [1781]) and the authors of *Constitutional Restoration* (*Grondwetige Herstelling* [1784–86]), were anything but democrats and preferred merely some influence from a broader segment of society than had been the case. As the Patriot Movement rolled to its denouement in 1787, some more extreme voices were heard, but I do not believe that these radical democrats were ever an important part of the movement and certainly not significant enough to justify the near-apoplectic response of Luzac.

I. LEONARD LEEB
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THEO BELL. *Divus Bernhardus: Bernhard von Clairvaux in Martin Luthers Schriften*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz,

Abteilung Religionsgeschichte, number 148.) Mainz: Philipp von Zabern. 1993. Pp. ix, 418.

Bernard of Clairvaux was an important exception to the disregard in which Martin Luther generally held medieval theologians. Luther viewed him, oddly, as the last of the great fathers of the church and held that, although Bernard lived at a time when the Antichrist had already gained power over the papacy, Bernard ranked second only to Augustine as a theologian and spiritual councillor. During the course of his career Luther referred to Bernard more than five hundred times and, despite a certain ambivalence, most of these references were overwhelmingly favorable.

Theo Bell's work, the first full-scale monograph on Luther's relation to Bernard, is a revision and German translation of his Dutch doctoral dissertation of 1989. Despite the revisions, traces of its origins as a dissertation remain. The work's methods and scope are narrow and modest; it is an explication of the references to Bernard in the writings of Luther. Occasionally it is repetitive and its organization pedestrian. But Bell also presents an exhaustively researched monograph that tells a sometimes fascinating story about the relation of two key thinkers in the Christian tradition.

Divus Bernhardus: the epithet was common at the turn of the sixteenth century, and Bell situates Luther's early appreciation of Bernard in a late-medieval context. Especially through the publication of his sermons, Bernard had become influential in a variety of circles, lay as well as clerical, as an alternative to the academic scholasticism that dominated theology. Bell correctly points out that we do not have at present a full picture of Bernard's late-medieval influence, but his reputation as a mystic, a humanist theologian, and a guide to monastic spirituality was the proximate background for Luther's initial fascination with Bernard.

The book's emphasis is on Luther's use of Bernard in the period to the end of 1521 and the publication of his *De votis monasticis iudicium*. References to Bernard in Luther's numerous writings after 1521 and in his "Table Talk" are dealt with summarily in the final two chapters. The core of the book examines in great detail and in chronological order the way that Luther used Bernard in the development of his own theology, in his mounting conflict with the papacy, and in his reassessment of the institution of monasticism. On all three of these key themes, Bell combines a command of the best recent secondary literature with an insightful investigation of Luther's lectures and writings. In Bell's view it would be too simple to speak of Bernard having determined or even directly influenced specific conceptual components of Luther's thought. Instead he prefers to speak of Bernard as a thinker who accompanied Luther on the course of his intellectual development and of a congeniality between the two rooted in a shared understanding of

the meaning of the Christian faith. With great thoroughness and skill this work both traces the shifts in Luther's image of Bernard and delineates the reasons for Luther's abiding admiration for him.

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HARRY OELKE. *Die Konfessionsbildung des 16. Jahrhunderts im Spiegel illustrierter Flugblätter*. (Arbeiten zur Kirchengeschichte, number 57.) New York: Walter de Gruyter. 1992. Pp. x, 474. DM 178.

Originating as a doctoral dissertation directed by Gottfried Maron, Harry Oelke's book can be situated among the intellectual progeny of Ernst Walter Zeeden, whose work on the rise and formalization of the major religious confessions during the sixteenth century continues to be influential. Oelke takes it for granted that his readers know that a confession amounts to more than a group's religious convictions. Wolfgang Reinhard's and Heinz Schilling's incorporation of the term into *Konfessionsbildung*—by which they mean the self-conscious shaping of a doctrinally distinct church and its deliberate exploitation by the state for its own purposes—has had the effect in recent historiography of lending the word an aggressive, even militant connotation.

Illustrated broadsheets (*Flugblätter*), in contrast to the multipaged early Reformation pamphlets, have been neglected by historians; for Oelke, however, they provide the means for tracing the complex evolution of beliefs into official creeds and creeds into instruments for gaining spiritual and temporal conformity. Printed on one side only, with pictorial and verbal messages that were comprehensible to a large popular audience, broadsheets were passed from hand to hand until they crumbled. Yet at least 788 have survived. This medium, first documented for 1423, reached its highest usage around 1580 and survived into the early nineteenth century. After 1530 censorship laws failed to stop the flow of unlicensed material. Oelke has most closely analyzed 141 broadsheets that explicitly deal with the age's religious struggles.

Although sensitive to the complicated interaction between the purposes of authors, artists, and printers, the perceptions of readers and hearers, and historical events, Oelke detects four phases in the predominant subject matter of the religious broadsheets: 1519–46, characterized by credal integration and polarization; 1547–55/60, dominated by Lutheran territorial self-definition; 1555/60–early seventeenth century, with an emphasis on the formation of confessionally distinct churches and their use of confessionalizing techniques; and 1580/1600–18, with sharpening politico-confessional divisions.

Early on Lutheran polemicists perfected mechanisms of biting satire that were quickly adopted by Catholics and, with some modification, that both

groups continued to employ in their reciprocal attacks throughout the century. A question Oelke leaves unanswered is why Calvinists made negligible use of broadsheets, although this only slightly lessens the interest of this study. From 1547, as secular governments exerted control over the churches, territorial princes and reformers were depicted together. Especially during Oelke's third phase, he sees the broadsheets as one means of carrying out what Gerhard Oestreich termed social disciplining (*Geist und Gestalt des frühmodernen Staats* [1969]). In order to present a united Protestant front to post-Tridentine Catholicism, Lutheran publicists spared Calvinists; they often poured vitriol specifically on the Jesuits. After 1580, a spirit of militancy prevailed on all sides. Perhaps ironically, some lay writers and artists from this period poignantly decried such unchristian disunity.

This book serves as a valuable reminder of the underutilized resources readily available for the scrutiny of this and other eras. Scholars will find a great many more uses for broadsides than they have to date. One wonders, nonetheless, why the consideration of single-sheet publications needs to be cleanly severed from that of pamphlets and books, both of which were often illustrated and may reflect a similar interaction between elite and popular concerns. It is especially unfortunate that the publishers could not afford to include high-quality illustrations. The earlier broadsheets, such as *Septiceps Luther*, are well known, but readers will very much want to examine the later ones. Even with a magnifying glass several of the texts are illegible.

The anglophone reader will note, too, that as plentiful as the documentation from the secondary literature is, Oelke makes little more than a gesture toward English-language works, assessing superficially the issues raised by Bob Scribner and omitting Miriam Chrisman when treating the output of presses in Strasbourg. (Kristin Zapalac's study of the use of images in Regensburg was published too late for Oelke to include.) These small cavils aside, this volume makes an indispensable contribution to our understanding of the processes of change and indocination after the first generation of the Reform.

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CARL C. CHRISTENSEN. *Princes and Propaganda: Electoral Saxon Art of the Reformation*. (Sixteenth Century Essays and Studies, number 20.) Kirksville, Mo.: Sixteenth Century Journal. 1992. Pp. viii, 149. \$35.00.

Carl C. Christensen's new book continues the author's longstanding interest in the imagery of the Reformation. His earlier work, *Art and the Reformation in Germany* (1979), is an indispensable survey of religious painting in this period, together with an excellent account of reformed attitudes toward images. In

this case, his focus is narrower; and he concentrates on reformed images that include portraits of the Saxon electors Frederick the Wise, John the Constant, and John Frederick. Christensen's book offers Reformation scholars a valuable account of the way religious and political values are manifested visually. It is in the discussion of individual works and the careful evaluation of their significance for the historical moment when they were produced that Christensen's contribution is greatest.

If the book has a drawback, it lies in the fact that its organizing idea, the notion that the images examined served as a kind of visual propaganda for the Saxon dynasty, is not sufficiently theorized. Christensen begins, for example, by examining altarpieces commissioned by the Saxon princes before the Reformation to show that their awareness of the propagandistic power of images antedated their conversion to the reformed cause. The inclusion of their portraits in altarpieces depicting devotional subjects, however, is just as easily explained in terms of late-medieval devotional practice than as a desire to promote the status of their own house. The donation of altarpieces was conceived as a good work that not only offered the ecclesiastical community an aid to prayer but also served as the means by which the donor could commemorate his or her personal piety. Those who venerated the images represented in these works were forced to recognize the donors' identities and to acknowledge their status as exemplars of Christian virtue. The special value of the altarpiece as a good work in an age obsessed with the idea of purgatory was not only that it characterized the donor as an eternal worshipper, but that the object itself became the focus of the unending prayers of others. It seems clear that in this instance the concerns of the Saxon electors were as much with their fates in the afterlife as with the promotion of their status in the present.

Dictionary definitions of propaganda suggest that it is a process involving the transfer of information from one location to another in order to promote the interests of the sender. This linear model of communication cannot cover the complexity of the value transactions in which Reformation images were involved. For example, in the case of Albrecht Dürer's portrait engraving of Frederick the Wise, we have no evidence that Frederick commissioned the artist to make the image. Although it is likely that Frederick sat for this portrait, it is by no means certain that he paid Dürer to make an engraving from the drawing. It is more likely that it was the artist rather than the ruler who took the initiative of translating the drawing into a medium capable of mechanical reproduction and putting it on the market. Did Dürer make this image so as to disseminate information that might promote the elector's cause? If he did, this was a rather roundabout way of doing so.

The attraction of an engraved image of Frederick the Wise, and the secret of its success in the marketplace, is that it made available an image of the prince

responsible for the protection of Martin Luther; the image was printed with a text that identified him as a defender of the faith. Who would have bought such an image? Surely it is more likely to have been those who were already converts to Luther's cause rather than those who opposed him. As Mikhail Bakhtin suggested, a message or utterance already incorporates its reception. At best the image serves to confirm an opinion already held rather than to create a new one. The process by which meaning is created would appear to be more circular than the linear model suggested by the notion of propaganda.

Christensen's model works best for those works specifically commissioned by the electors for distribution to their aristocratic followers, such as the sixty portrait pairs representing Frederick the Wise and John the Constant ordered by John Frederick from Lucas Cranach the Elder at the beginning of his reign. These portraits were accompanied by inscriptions attributed to Luther that celebrated their capacities as rulers but above all their roles as protectors and promoters of the Reformation. The function of these portraits appears to have been to link the fate of the secular rulers of Saxony together with that of the leading spiritual reformer of the age so as to enhance their status in the eyes of the converted. Even in this instance, however, one is compelled to ask whether secular and religious interests can be sufficiently distinguished to justify interpreting these images as dynastic rather than religious propaganda.

In his conclusion Christensen draws an analogy between the imagery depicting the Saxon princes and that produced by Maximilian I as a memorial to himself. Far from suggesting a parallel, the analogy points out the differences between the two genres of images. Whereas the Saxon princes are represented as secular rulers involved in the promotion of a spiritual message, the imagery of Maximilian I makes reference only to his own glory. Maximilian is portrayed as a chivalric hero, Roman emperor, and so forth, not in the interests of some transcendent ideal but to record his own virtues and accomplishments. Whereas Maximilian put himself first, the Saxon princes subordinated their role to that of the great spiritual leader of the time. As a consequence, I suggest that Christensen's book may be more usefully viewed as a contribution to our knowledge of Reformation imagery rather than of imagery produced in the service of a secular dynasty. Regarded in this light, the work offers compelling interpretations of subjects that have great relevance and bearing on our understanding of the role of images in the making of the Reformation.

KEITH MOXEY
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MACK WALKER. *The Salzburg Transaction: Expulsion and Redemption in Eighteenth-Century Germany*. Ithaca: Cornell University Press. 1992. Pp. xvi, 242. \$34.95.

Mack Walker's fourth monograph is a welcome new book from a scholar who always challenges us to rethink historical problems. Never happy with overly neat interpretations or complacent about his own, enjoying an awesome command of source materials and secondary research yet uncommonly modest about his knowledge and accomplishments, Walker has written and taught for some thirty years with an exemplary emphasis on substance over style, historical complexity over outworn formulas, and commitment to scholarship over careerism.

In this volume Walker explores the notorious expulsion of 20,000 Protestant peasants from the archbishopric of Salzburg in 1731–32, often considered an egregious instance of bigotry running counter to the longstanding tendency toward religious toleration in the Holy Roman empire. In Walker's analysis, however, this seemingly straightforward event becomes a complex transaction, and he skillfully reconstructs the several actors' different perspectives and roles in the negotiated resettlement of the Salzburg emigrants to East Prussia. Without recourse to post-modern interpretive theory or elaborate methodological discussion, Walker offers us five distinct but interlocking accounts that reconstruct a single historical episode.

The first narrative presents the most revisionist interpretations, for Prince-Archbishop Leopold Anton von Firmian turns out to have been not a retrograde fanatic but rather a cultivated although politically naive prelate whose Jesuit-influenced educational and pastoral reforms engendered opposition from his own clerical establishment. This confrontation made accommodation with heretical mountain folk virtually impossible; outside Protestant support stiffened peasant resistance, turned it toward civil disobedience, and gave the archbishop justification for moving quickly to expel religious dissidents.

The plot shifts next to Frederick William I of Prussia and his invitation to the Salzburger to settle in his kingdom, just as he was expelling Mennonites for their refusal of military service. Walker emphasizes the king's cameralist population policy over his piety in a transaction of considerable propaganda value, aiding the soldier king to assume political leadership of mainstream Protestants in the empire. His harmonious but indirect negotiations with Firmian required a broker, the special role of the emperor in German politics. Most impressive in Charles VI's story, the third perspective presented here, was the emperor's skill at balancing particular Habsburg interests with those of his fellow princes, so that all parties gained something in 1732. In linking the major issues of the year—the Industrial Trades Ordinance, the Pragmatic Sanction, and the Salzburg Transaction—Walker handsomely demonstrates the efficacy of imperial institutions.

The fourth account focuses on the migrants themselves, of whom thirty families form a core group (not representative but illustrating a range of social possi-

bilities) for Walker's closer study of debt patterns, the dependent or "undomiciled" population, and the change from relative domestic isolation and autonomy in alpine Salzburg to greater communal and state influence on family relations in the Prussian flatlands. This story and the final one are too short, given their high potential for social and cultural analysis. Perhaps most fascinating but also most frustrating is the concluding discussion of the legends that grew up around the Salzburg exiles: their transformation into exemplars of evangelical piety and virtue and later of economic enterprise and Prussian discipline. Their invented history became part of confessional rivalries and then *kleindeutsche* politics from the eighteenth into the twentieth century, and I suspect readers will find themselves wishing for more than the insightful but sketchy account here. Specialists could raise similar regrets over unexplored subjects or comparisons not made in each narrative Walker undertakes, although research losses on individual topics must be weighed against the chief value of this study: the diversity of perspectives offered by a fine historian who, after thirty years, remains stimulating and fresh.

GERALD L. SOLIDAY
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WOLFGANG NEUGEBAUER. *Politischer Wandel im Osten: Ost- und Westpreussen von den alten Ständen zum Konstitutionalismus*. (Quellen und Studien zur Geschichte des östlichen Europa, number 36.) Stuttgart: Franz Steiner. 1992. Pp. vi, 551. DM 198.

In the broadest terms, this detailed study of political life in the provinces of East and West Prussia (later combined into the single province known simply as Prussia) from the later eighteenth century to the 1840s demonstrates that the emergence of modern forms of representative government in the nineteenth century did not always follow some variant of the French Revolutionary model. Change did not always involve the repudiation of older forms of representation that existed in the old regime, but it could develop from within such older forms. In making this claim, Wolfgang Neugebauer also raises the possibility that the evolutionary continuity of traditional representative bodies may have been demonstrably important for progressive political change in other German states of the period. Further, given the overwhelmingly agrarian character of the Prussian provinces, he questions whether the stimulus for political modernization is as necessarily related to early stages of industrialization as conventional wisdom would have it.

At issue here is the contribution of the East and West Prussian estates (*Stände*) to the growth of new concepts of political participation grounded not on the basis of exclusive social orders defined by birth

but rather on the principle of inclusive common interests peculiar to the region and on the desire to protect and advance those interests. These interests were to a large extent economic, increasingly determined by external market and monetary forces (a form of "westernization" [p. 486] of the area in Neugebauer's terminology). These economic interests led to a growing recognition on the part of noble landowners that their concrete interests as landed proprietors were more important in the long run than their increasingly immaterial status as aristocrats and to their identification with the growing numbers of bourgeois estate owners; this eventuated as early as 1800 in petitions to the crown to include such owners as members of the noble estate. At the same time, close and longstanding ties of common economic and political interest with the bourgeois elites of the cities were further strengthened, helped along by the eager reception of new philosophic and economic doctrines such as those of Immanuel Kant and the Smithian economist C. J. Kraus.

Although the advancing identification of common economic interests among all the important elites of the province greatly increased their political potential, the possibilities for realization of that potential were based on political cooperation. This in turn depended on both informal and institutional structures and processes that stretched as far back as the seventeenth century and on a regional self-consciousness that had always been strong in these eastern areas of the monarchy. Neugebauer makes it clear that a traditionally independent political mentality was fundamental to all of the changes he describes, and in doing so he emphasizes the limitations of any approach to Prussian history that tends to overestimate the leveling or homogenizing influences of a supposedly monolithic absolutist bureaucratic state.

Thus, in contrast to Brandenburg, where the provincial estates supported political reaction both before and after 1815, the Prussian estates, frustrated by the repeated failure, given their purely advisory role in legislation, to modify the central government's alternately invasive and insouciant policies relative to their local concerns, began to withdraw from administrative functions in the state as early as the late eighteenth century. They increasingly repudiated the whole concept of a partnership in legislation with the Berlin government and eventually, by the eve of the 1848 revolutions, adopted an oppositional stance suggestive of the separation of powers in a constitutional monarchy, in effect joining the liberal movement of the 1840s. To be sure, the rhetoric justifying the estates' evolving political views and actions often invoked representative traditions of the old regime, but Neugebauer insists that the force of those traditions was by no means merely reactive. Under changing internal and external conditions it could and did lead forward to an entirely new approach to the

problem of bringing society and government closer together.

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ERIC DORN BROSE. *The Politics of Technological Change in Prussia: Out of the Shadow of Antiquity, 1809–1848*. Princeton: Princeton University Press. 1993. Pp. 290. \$39.50.

Although the historiography of modernization seems in eclipse, Eric Dorn Brose's fine study shows that much worthwhile work can still be written. Brose exposes the complexities of Prussian modernization during the critical first half of the nineteenth century while avoiding unneeded detail. He states at the outset that "competing intrastate agencies, cultures and party-political factions with different ideologies and agendas" (p. 3) make generalizations difficult, but he nevertheless clearly examines the nature of the state, its interaction with nonofficials, and its contribution to modernization. Although based primarily on official sources, this is not just another history of the Prussian bureaucracy, for the reader is taken through a broad swath of political, cultural, and technological history, connecting officials, soldiers, civilians, and businessmen, each with their own interests. A key theme is the conflicting visions of the future during the *Vormärz* period, which produced a crisis in state and society only resolved after 1850. When seen from the perspective of its manifold bureaucratic-civilian links, the authoritarian state looks rather different, a system that connected individuals from the absolutist monarch down to miners, craftsmen, and producers.

Six individual chapters cover various agencies, coteries of like-minded people, revealing their hopes, dreams, and projects. The scene is set with the attempts of the "bourgeois king" Frederick William III and his economically liberal chancellor Karl August von Hardenberg to discourage urban manufacturing by favoring the establishment of rural factories. The king and his minister retreated when challenged by antimodernist opponents.

The early liberals, especially those in the Prussian military, have been studied by others, but Brose sets them in a larger context. They had their own reform agenda of constitutionalism, humanistic curricula, and a free Germany, but by the 1830s they were in disarray. Less well known than Carl von Clausewitz and August Wilhelm von Gneisenau was Peter Beuth of the business department of the finance ministry. He expounded an aesthetic future. A decade after their visit to industrializing England, his friend Karl Friedrich Schinkel painted him soaring above a factory town on Pegasus. Increasingly elitist (and anti-Semitic), Beuth's search for entrepreneurs willing to combine new technologies with a devotion to art

failed to attract businessmen, who began to organize their own technological societies.

The Royal Mining Corps (*Oberhauptmannschaft*) also had a mixed vision. It first attempted to restore centuries-old mining and metallurgical techniques to their previous glories, then yielded to the forces of change. Once again Brose skillfully merges technological history, political conflict, and macroeconomic forces in fascinating detail.

In the chapter "Spartans of the North," Brose focuses on the technological development of the army. After the exhausting Napoleonic age Prussia lacked the funds to modernize its arms, but he argues that the late 1820s marked a watershed as pragmatic officers emerged who were more interested in technical subjects. Brose also examines the Masonic vision of Christian Rother, head of the Overseas Trading Corporation (*Seehandlung*), as the agency expanded beyond its initial charge of making foreign purchases and organizing finance for the government to become a significant instrument of state industrial investment. Finally came the railroads, another tangled story of false starts, indecisive government policies, military opposition, and final acceptance. In time railroads became a key part of Prussia's economic takeoff.

Brose questions whether the Prussia he presents was a state either controlled by the aristocratic Junkers or enthralled by a bureaucratic *Beamtenstand*. The Junkers were declining economically and lacked the king's sympathy; their best talents turned elsewhere, while the bourgeoisie permeated the state. But intra-agency fragmentation and the persistence of class divisions among the civil servants, the many ties between bureaucrats and entrepreneurial elites, and the variety of splits among the bourgeoisie indicate that Prussia was no *Beamtenstaat* either. Brose explains the monarch's power by arguing that he "schemed" against two loose party blocs to preserve his personal power (p. 265). This conclusion deserves serious consideration, although it will not win ready assent. Nonetheless there is much to be learned from this book, and it should find its way onto undergraduate reading lists.

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RAYMOND H. DOMINICK III. *The Environmental Movement in Germany: Prophets and Pioneers, 1871–1971*. Bloomington: Indiana University Press. 1992. Pp. xiii, 290. \$35.00.

Environmental history emerged as an important field of study in the United States over two decades ago, but European scholars have been slower to recognize its importance. Germany's environmental historians—Arne Andersen, Franz-Josef Brüggemeier, Thomas Kluge, Thomas Rommelspacher, Engelbert

Schramm, Rolf Peter Sieferle, Gerd Spelsberg, Klaus-Georg Wey, and a handful of others—have begun to fill that gap, but their works are largely inaccessible to English-language readers.

Raymond H. Dominick III's fine book is an especially welcome addition to this new field. He concentrates primarily on the German environmental movement up to the emergence of the Greens in the 1970s. His book is organized loosely around the sociological theories of new social movements and resource mobilization. It focuses on a wide variety of governmental and nongovernmental organizations (such as the Bavarian State Committee for the Care of Nature, the Prussian Governmental Center for *Naturdenkmalpflege*, the German League for Bird Protection, the Nature Park Society, and the League for Conservation in Bavaria), as well as ad hoc NIMBY (Not In My Back Yard) movements that appeared and reappeared as various local environmental causes waxed and waned.

The greatest strength of Dominick's book lies in his discussion of environmental politics in the Second Reich (1871–1918), when Germany emerged as a leading industrial power. Environmentalism, he argues, was largely a middle-class movement, dominated by government bureaucrats and academics. He offers a socioeconomic profile of the membership and an analysis of the influence of various environmental groups, utilizing a number of excellent graphs (for instance, a graph of newspaper coverage on environmental issues in the *Frankfurter Allgemeine Zeitung* from 1872 to 1913) to illustrate his main points.

Dominick sketches the ideological roots of environmentalism through the writings of naturalists such as Alexander von Humboldt, agrarian romantics such as Ernst Rudorff, and *völkisch* nationalists such as Wilhelm Heinrich Riehl. Refuting Anna Bramwell's claim (*Blood and Soil* [1985]) that the origins of the Greens can be found in the ideology of the German Right, he argues that conservation groups experienced the same *Gleichschaltung* process as other organizations in Nazi Germany. Of Germany's eighteen most prominent conservationists, only Paul Schultze-Naumburg joined the Nazi Party before 1933, while another nine became members thereafter. Most nature organizations were incorporated into Nazi organizations (usually Hermann Göring's Forest Ministry or Robert Ley's Labor Front), but the Left-leaning Friends of Nature (with 100,000 members) was dissolved and banned by the Nazis.

Dominick's book accords more space to the Second Reich than to later periods of German history, focuses more on agrarian Bavaria than on the industrial Ruhr, covers nature conservation more than pollution abatement, and tends to neglect East Germany in its discussion of the post-1945 era. Readers primarily interested in the Green Party will be disappointed. Access to archives partially explains his emphasis on certain regions and topics, as does his interest in the earliest roots of German environmentalism over con-

temporary politics. The net effect, however, is that he paints a picture of German environmentalism that seems overly rural and antimodern, despite his best efforts to stress its urban and modern roots.

If Dominick's book falls short of a definitive history, it is largely because he undertook the impossible task of writing a comprehensive overview before other scholars had created the monographic literature on which he could build. He has, however, greatly eased the work of future researchers. This book will remain indispensable reading for many years to come.

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JOST HERMAND. *Old Dreams of a New Reich: Volkish Utopias and National Socialism*. Translated by PAUL LEVESQUE and STEFAN SOLDVIERI. Bloomington: Indiana University Press. 1992. Pp. xvi, 332. \$35.00.

This book, first published in German under the title *Der alte Traum vom neuen Reich: Völkische Utopien und Nationalsozialismus* in 1988, offers a comprehensive account of the transformation of utopian nationalist thought in Germany from its democratic origins in the Enlightenment to its fascist corruption in National Socialism. Jost Hermand rejects the linear, mechanistic approach common to many previous histories of "volkish" ideology in favor of a dialectical analysis that closely relates these ideas to social and political conditions and to the concrete interests of their propagators. By carefully differentiating between genuine utopian aspirations and propagandistic manipulation of ideas of national community, he makes the rise of National Socialism seem less inevitable or predetermined than accounts that focus solely on the literal parallels or superficial continuities in nationalist discourse in Germany over the last two centuries.

Hermand's thesis is that legitimate desires, hopes, and yearnings originally inspired the quest for a national "sense of community" in Germany, and his aim is to "extricate this often-abused term from the ideological bankruptcy of German fascism and restore some of its earlier luster" (p. xiv). He criticizes thinkers of the Left for repudiating the idea of the national state, thereby ceding nationalist and communitarian ideals to the Right. His strictures, formulated in response to the challenge of West German neoconservative revisionism in the 1980s, remain highly relevant in a reunited Germany in which right-wing ideologues once again seem prepared to "use a vague yearning for a sense of 'national identity' as a means of masking the actual injustices of society" (p. xvi).

The history of modern German nationalism, although it was never a monolithic movement, offers a classic example of the transformation of essentially democratic and emancipatory values into an ideology

of domination, aggression, and oppression. Hermand's nuanced analysis of a wide variety of texts shows how the apparent continuity in "volkish" ideas from the late eighteenth century to the early twentieth century conceals important differences deriving from the different political contexts in which these ideas were espoused. The ideal of a united German fatherland was born in opposition to the rigid absolutism and elitism of the German states. Even after the Napoleonic wars reinforced the traditionally anti-French impetus of the German national movement, a predominant libertarian spirit of popular sovereignty "makes it almost blasphemous to speak of the nationalist tendencies of this era in terms of a prefascism or protofascism" (p. 13).

Hermand traces the radicalization and exploitation of nationalist aspirations in political manifestoes and literary works through the familiar phases of German history from unification to the end of the Nazi era. He locates a significant shift to protofascism in the antiproletarian, Social Darwinist tracts of the popular opposition to the relatively moderate Wilhelmian policies of the 1890s and 1900s. Particularly suggestive is his analysis of the shift in emphasis around the turn of the century from dystopian denunciations of proletarians as subhumans to utopian schemes to integrate the German worker in a new *Volksgemeinschaft*, a shift accompanied by a growing tendency to blame class divisions on Jews.

Hermand, coauthor with Richard Hamann of the magisterial four-volume *Epochen deutscher Kultur von 1870 bis zur Gegenwart* (1965), has uncovered an extraordinary number of "volkish" utopias, some uncannily prescient of the coming fascist regime. His examples are drawn from refined literary movements such as the Stefan George circle, explicitly political writings, and long-forgotten popular works, some of which, because of the postwar purge of Nazi literature, have survived in only a handful of hard-to-find copies. Ironically, the very mass of Germanophile materials, untempered by any discussion of countervailing influences, tends to reinforce the illusion of an irreversible momentum toward National Socialism that Hermand wishes to dispel. Almost half the book is devoted to the Nazi period itself, in which utopian literary projections were firmly harnessed to the regime's imperialistic aims.

The book contains a large number of excellent illustrations. Its clear, accessible style makes it well suited as a supplemental text in German history courses. Translators Paul Levesque and Stefan Soldovieri have done an admirable job of simplifying the German syntax in the interests of lucidity. They are adept at choosing the words or phrases that best approximate the meaning of the original. Only rarely do they resort to artificial constructions such as "cultification" (for the almost equally artificial *Verkultung*). Their rendering of the DNVP as the German National Volkish Party (rather than the more usual German National People's Party) seems appropriate

in view of that party's predominantly "volkish" ideology.

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GARY A. ABRAHAM. *Max Weber and the Jewish Question: A Study of the Social Outlook of His Sociology*. Urbana: University of Illinois Press. 1992. Pp. xii, 319. \$34.95.

Popular opinion to the contrary notwithstanding, Max Weber not only was not a philo-Semite, but had anti-Semitic leanings. That is the opinion of Gary A. Abraham, who argues that, for all his liberalism and despite his personal efforts on behalf of individuals who suffered anti-Semitic discrimination, Weber was no friend of the Jews, the world's "pariah people." It is partially from his use of that phrase that Weber's true opinions regarding Jews can be inferred, according to Abraham.

Weber, like many liberal Germans of the time, was before all else an impassioned supporter of Germany. That support demanded acceptance of a single German culture, one that should not tolerate ethnic or religious minorities. Thus, while Jews as people caused no problems for Weber or for other German liberals, Jews as Jews did. To German liberals Jews were acceptable only insofar as they recognized that they would not long remain Jews.

Weber, according to Abraham, viewed the pariah status of the Jews as a condition of their own making. For two millennia Jews chose to remain isolated from the majority of society, their choice in large degree a consequence of the demands of their religion. Thus, although their continued separation from German society was partly due to anti-Semitism, in the main it resulted from their refusal to abandon their self-segregation.

Weber's view of the Jews was altered as a consequence of his reading of his more anti-Semitic contemporaries. Abraham notes especially the writings of Werner Sombart. Where Weber initially offered the view that Judaism is a contemplative religion, he was moved later to argue that a drive to success was basic to Judaism, that is, that Judaism is "rational" (p. 221). In this context, "rational" means amoral.

Weber presumed to have endorsed a value-free approach to scientific research. Yet, according to Abraham, he did not truly use such an approach in his own work. The quest for a "universal history" (p. 228) that informs all his works was humanistic on the one hand but culturally restrictive on the other. It directed his determination of who should be included in and who should be excluded from German society.

Abraham notes that Weber's opinions of Jews and of Judaism were ambivalent. Weber understood that Jews lived in a hostile social environment and to some degree sympathized with them. But he was aware too of the Nietzschean view of *Ressentiment* (p. 249) and

considered it quite plausible that Jews were antagonistic toward their hosts.

Abraham's book is based on his doctoral dissertation, and at times this shows. There is a pedantic quality to his work. For example, in just over 300 pages of text there are 770 footnotes. Abraham can also be faulted for setting up straw men on occasion, as when he insists that Weber's sociology of the Jews is not separate from his broader interests in history and sociology. Whoever said that they were separate or implied that they should or even could be?

Part of the problem with Abraham's thesis is that Weber never dealt directly with the "Jewish question" and indeed seemed to have deliberately avoided direct discussions of contemporary Jewry. The book, then, is necessarily based on interpretations of Weber's tangential writings regarding the Jews. Moreover, Abraham is in the position of judging the ethics of a man and his writing nearly a century after the fact, and that is worrying. The meaning of "liberal" has changed significantly only recently. Surely people living at the turn of the twentieth century would have had different "liberal" views than those living at the turn of the twenty-first century. Abraham is on shaky ground in judging Weber's liberalism from the perspective of the 1990s.

Abraham's arguments are solid and well researched (the 770 footnotes attest to this). To accept those arguments and his conclusion, however, one has to accept a continuity of social definitions and social ethics in the years since Weber's death.

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CHRISTIAN JANSEN. *Professoren und Politik: Politisches Denken und Handeln der Heidelberger Hochschullehrer 1914–1935*. (Kritische Studien zur Geschichtswissenschaft, number 99.) Göttingen: Vandenhoeck und Ruprecht. 1992. Pp. 414. DM 68.

CHRISTIAN JANSEN. *Vom Gelehrten zum Beamten, Karriereverläufe und soziale Lage der Heidelberger Hochschullehrer 1914–1933*. Heidelberg: Wunderhorn. 1992. Pp. 179. DM 28.

A German doctoral dissertation that fills not one but two books one approaches with trepidation, fearing interminable pages of indigestible prose. With Christian Jansen, nothing could be further from the truth.

Jansen sets out in his stimulating book *Professoren und Politik* to construct a "social history of ideas" (p. 14) of the Heidelberg faculty in the two decades prior to the Nazi period. He undertakes an exhaustive analysis, charting the political affiliation of faculty during five distinct time periods between 1914 and 1945 (neatly summarized in a table; *Professoren*, pp. 394–99). Similar studies have tended to focus on prominent individuals or to assess overt political activity. Jansen goes in search of all expressions of the political views of every faculty member he can find.

He investigates the professors' membership in political parties and their ex-cathedra pronouncements, but beyond that he has, for example, combed the local press for their commentaries on current affairs. Some 40 percent of the faculty appear in the 800 articles and speeches Jansen managed to unearth. Another indication of political thinking comes from public declarations professors signed. In a comprehensive table (*Professoren*, pp. 400–06), he shows which of some thirty resolutions between 1914 and the end of 1932 each faculty member signed.

Jansen's research refutes the accepted view that only 10 to 15 percent of German professors engaged in political activity. He makes no claim for the typicality of Heidelberg; rather, he repeatedly shows that it departed from the norm. This nevertheless represents an important case study because the university remained the only one, aside from Frankfurt, dominated until 1930 by faculty who displayed either a neutral or a positive attitude toward the Weimar Republic.

The reticence of the Heidelberg faculty to damn the republic is curious; even right-wing professors withheld their criticism until 1923, a courtesy not extended by their counterparts elsewhere. The explanation seems to lie in their loyalty to the state rather than in any sympathy for republicanism. A blind trust in the ability of those charged with running the country eventually to put things in order led to an underestimation of the danger posed by the Nazis. After student riots in 1931, Karl Jaspers was asked: "If the house is burning, won't you help to put it out?" His reply was: "No, then I shall call the fire brigade" (p. 89). Not until 1945 did he acknowledge this to have been a political error.

Yet Jansen confirms Konrad Jarausch's claim that "surprisingly creative" political ideas permeated the educated bourgeoisie in the 1920s (p. 188). This does not mean that the professors approved of the cut and thrust of a pluralistic political system. Politics was not for them an attempt to find compromises between different interests and values. Rather its purpose was to translate into action throughout society a political concept they endorsed as valid. This sovereign attitude buckled with surprising ease under the economic crisis and the violent radicalization of the campus after 1930.

In the end, most professors came to embrace the new Nazi regime with an enthusiasm Jansen finds unforgivable, for it initially required only silence from them. He explains this reaction by their penchant for charismatic forms of authority and by their ineptitude, despite their ideas, for independent, collective political action. This is, then, a book with a depressing ending, as Jansen demonstrates how easily the famous liberal spirit of Heidelberg evaporated. In a crisp conclusion, he posits nine theses about the reaction of liberals to political conflicts.

Included among Jansen's theses is the feeling of marginalization among professors. In *Vom Gelehrten*

zum Beamten, Jansen explores the question of the faculty's social and economic status more fully. He offers ample proof that, wherever professorial worries came from, they had no genuine basis in fact. He convincingly refutes Fritz Ringer's assessment (*The Decline of the German Mandarins* [1969]) of their declining economic status. In the most exacting study to date of faculty salaries, appointments, and promotions, Jansen proves that they actually did better than ever in the Weimar era. Junior faculty reached their prewar salary levels as early as July 1923. Moreover, between 1927 and 1930, a period of stable costs of living, some 75 percent of tenured professors were able to increase their monthly salary by 200 Marks, roughly the equivalent of a worker's full wage. Only the Great Depression put an end to this, and even that cataclysm needs to be seen in perspective. Although salaries were cut an average of 21 percent, the cost of living fell by 30 percent, more than cushioning the loss.

For the period of the Third Reich, too, Jansen produces surprising evidence: it was not primarily Jewish professors, but politically obstreperous ones who were punished the most with salary cuts. Two professors eventually sacked as "non-Aryans" actually improved their salaries after 1933. Although Jansen does not say so, this tends to support a functionalist interpretation of Nazi university policy.

Weimar academics, then, led a relatively charmed existence in career terms. The average age at which the youngest generation gained doctorates sank to twenty-four, and they gained both academic appointments and promotions earlier. That 63 percent of the *Habilitierten* went on to achieve a full professorship gives the lie to Max Weber's contemporary warning (*Wissenschaft als Beruf* [1919]) that it was simply irresponsible to encourage a young man to aim for an academic career in those uncertain years.

Vom Gelehrten zum Beamten is admittedly a subsidiary volume to *Professoren und Politik*, but it contains valuable information that considerations of space kept out of the other book. Future historians of German universities in this period will want to consult Jansen's extensive bibliography of writings by and about each Heidelberg faculty member, including all the ephemeral political articles that he gleaned from the local press (*Gelehrten*, pp. 122–63). In these two books, Jansen revises some of the classic studies on the topic. His writing is marked by good judgment and an extraordinary thoroughness. This young Bochum historian is clearly a scholar to watch.

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GERHARD SCHULZ. *Zwischen Demokratie und Diktatur: Verfassungspolitik und Reichsreform in der Weimarer Republik*. Volume 3, *Von Brüning zu Hitler: Der Wandel des politischen Systems in Deutschland 1930–1933*. New York: Walter de Gruyter. 1992. Pp. xvi, 1101. DM 398.

When contemplating reading a book of more than a thousand pages devoted to the last three years of the Weimar Republic, it is difficult to avoid the mental reservation of deciding whether a work of this length on one of the most widely researched periods in German history is really a worthwhile undertaking. Can there be new discoveries or an original interpretation of the events that led up to the Nazis' coming to power? After finishing Gerhard Schulz's book I am happy to say the answer is decidedly yes. This volume, the last of the author's trilogy on the political history of the Weimar Republic, was definitely worth waiting for.

The book's primary significance lies in the originality of Schulz's methodological and conceptual approach. The title is a little misleading (some 850 pages are devoted to Heinrich Brüning and his chancellorship, Franz von Papen and Kurt von Schleicher are covered in fewer than 200 pages, and the author summarizes the last dramatic weeks in January 1933 in some twenty pages), but this seeming distortion in coverage is fully justified by Schulz's unique view of the historical dynamics. In terms of the fashionable division of historians of modern Germany into "personalists" and "structuralists" the author is a personalist, but with a rather unique twist of his own. This is history from the top down, but the decision makers act not only as agents of power but also become vivid personalities, whose human foibles directly influence their political and economic decisions. This is also a decidedly anti-historicist book. Those who made contemporary decisions stand in the limelight, rather than those who, with the benefit of hindsight, we know will be important at a later stage. There is relatively little on the Nazis until page 522, and Adolf Hitler does not appear at the center of events until the last weeks of the Weimar era. Until then Schulz portrays the Nazi leader as an object of decisions by those who led the presidential regimes.

In consequence of his personalist approach, Schulz's central thesis blames Germany's leaders for the destruction of the Weimar Republic. According to the author, Germany "slipped" (*schliefte*) into the presidential system and eventually the Nazi dictatorship, because "between Brüning and Hitler in the great German people no power and no person of influence and stature was able to point the nation into another direction or even attempted to stop the seemingly inescapable evolution" (p. 1049). In other words, the failure of Weimar was a failure of men, not a failure of systems or the consequence of a historical *Sonderweg*.

If the failure of Weimar was a failure of men, then the politician bearing primary responsibility was, of course, Brüning. Schulz probably provides the most balanced and also the most devastating analysis of Brüning that can be found in the extensive literature on the Weimar era. According to Schulz, Brüning failed both conceptually and as a person. The chancellor recognized that the Weimar crisis was primarily

a financial and economic problem, but he insisted that the solution lay in foreign rather than in domestic policy successes. Despite mounting evidence to the contrary, Brüning persisted in pursuing a *Primat der Aussenpolitik*; he was convinced the end of Germany's reparations, and only that development, would result in domestic stabilization, the return of Germany's great-power status, economic prosperity, and, more incidentally, constitutional reform. This one-sided plan was conceptually flawed, but Schulz is equally hard on Brüning as a person. The chancellor simply did not have the qualities of character required of him. In Schulz's telling if rather old-fashioned terminology, Brüning did not have the "Nervenkostüm" (p. 843) to be a good crisis manager.

After analyzing in great detail Brüning's quest and failure, Schulz treats his successors, Papen and Schleicher, as mere bit players. His book suggests that Brüning is at least worth close analysis; Papen and Schleicher are not. Neither of Brüning's successors, according to Schulz, had a clear conception of what had to be done, and Papen, unlike Schleicher, who pursued activism for its own sake, was not even active during his tenure as chancellor. As for Hitler, this book very much cuts him down to size. Schulz's personalist approach works particularly well in analyzing the negotiations that brought Hitler to power. It is the foibles of his partners, rather than the skill of the Nazi leader, that led to the Nazi *Machtergreifung*.

This is a work in which economic and foreign policy developments take center stage, and it is not surprising that Schulz has some decided heroes and villains among Weimar's crisis managers. Fritz Schäffer, the finance minister in Brüning's first cabinet, definitely belongs in the category of heroes. According to the author, Schäffer was one of the few who had a clear understanding of the origin and development of the economic crisis and its political implications. Among the large group of rather mediocre industrialists, Paul Silverberg, the Ruhr *Macher*, stands out as someone who looked beyond government subsidies and worked hard to salvage the Weimar political system. Among the villains, the presence of Robert Curtius, the foreign minister, may be somewhat surprising. Schulz sees him as a weak personality, unable to resist the dominating influence of the Reichswehr on Germany's foreign relations. In fact, for Schulz the Reichswehr and especially Schleicher, here in his capacity as a military professional not chancellor, more than any other factor or individual destroyed Weimar Germany. Schulz provides a devastating portrait of the military leadership determined to use Brüning's *Primat der Aussenpolitik* to prepare the way for German rearmament, without seeming to realize that their single-minded pursuit of this goal destroyed any possibility of Brüning's succeeding in his foreign policy quest.

In this book, the last years of the Weimar era are masterfully woven into a brilliant narrative to which a brief review cannot possibly do justice. Schulz is

particularly effective at interlacing the massive secondary literature with his own archival researches. True, there are no dramatic new discoveries in this book, but there are nuances and especially a degree of originality of synthesis that has not been achieved in previous works.

There are items, however, that suggest criticism. For someone familiar with the American literature on the subject, it is disappointing to find a number of important English-language works missing from the bibliography. On some of the events covered only specialists will find the details of interest. And structuralists and practitioners of *Alltagsgeschichte* will not agree with Schulz's personalist approach from the top down. For much of the book the elites are among themselves. The army of unemployed Germans remains a statistic, and the endemic political violence that engulfed Germany for much of the period covered here appears largely as background information for the process of ministerial decision making.

Still, these are small caveats that in no way detract from the brilliance of the analysis. With this work Schulz has given us the crowning achievement of his long years of labor on the history of the Weimar era. I, for one, am delighted to see that someone is still writing history like this.

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WALTER RINDERLE and BERNARD NORLING. *The Nazi Impact on a German Village*. Lexington: University Press of Kentucky. 1993. Pp. 276. \$33.00.

The 1,600 inhabitants of Oberschopfheim, a village near Lahr in the southwestern German state of Baden, seem scarcely to have heard of the Nazi movement as late as 1928. Yet two years later 28 percent of these overwhelmingly Catholic peasants voted for Adolf Hitler; in March 1933 almost half did so. What accounts for the rapid transformation in electoral allegiance of this former "black nest of the Center Party" (p. 107)? According to Bernard Norling and Walter Rinderle (whose father was born in the area), the reasons were both diverse and symptomatic. Beginning with the imposition of production and price controls on food by the imperial regime during World War I, Germany's farmers were progressively alienated from the urban-oriented governments ruling from Berlin. The successive disasters of rampant inflation and the Great Depression, which destroyed agricultural markets, completed the disillusionment with Weimar democracy. Hitler's vague and contradictory promises to solve the crisis by introducing a regulated economy and massive distribution of financial aid complemented the "generally accommodating" attitude of Freiburg's Archbishop Conrad Gröber toward Nazism (pp. 96, 103-04, 122).

The seizure of power and the ensuing dictatorship,

however, left little imprint on Oberschopfheimers. The insignificant numbers and geographical isolation of the villagers preserved them from Nazi depredations, as did the fact that no Jews and only two Communist voters lived among them. A resolute parish priest—the sole rounded character presented in the book—warded off intermittent anticlerical forays by local party leaders. Although battlefield casualties among draftees from the village were appallingly high, Oberschopfheim itself was barely damaged in the war and its population remained loyal to church and *Führer* until its overburdened women “at gunpoint” (p. 176) compelled Nazi officials to surrender to advancing French forces on April 16, 1945.

This very readable story emphasizes continuities within change in German historical development during the twentieth century. Empathetic discussions of rural labor conditions and the village’s administrative self-management preceding the Nazi era, coupled with anecdotal reportage about the real revolution that postwar demographic, cultural, and occupational alterations brought to Oberschopfheim, accurately reflect the limited impact of Hitlerism on such basic aspects of German life as the birth rate or religious convictions.

Therein also lie weaknesses in interpretation and research by Rinderle and Norling. For one thing, the presence of conscripted Polish farm hands is said to have led to “embarrassing” pregnancies and (with the wartime shortage of doctors) increased miscarriages among rural women (p. 171), but no mention is made of the usually murderous punishment inflicted on Poles for such sexual behavior. Often the prejudices of the unidentified interviewees toward socialists, “guest workers,” refugees, and others are reported without critical analysis; indeed the authors appear to share their opinionated views of intellectuals and especially Marxists. The resources of the Berlin Document Center on Nazi Party membership are mistakenly overlooked, and a twice-mentioned appendix detailing the composition of Oberschopfheim’s own unit is inexplicably omitted. Informative, wide-ranging comparisons with the experiences of other communities in France, Greece, and elsewhere mask a failure to consult the German-language literature about similar locales within the Reich. Overall, this is a worthwhile but flawed study.

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VALDIS O. LUMANS. *Himmler’s Auxiliaries: The Volksdeutsche Mittelstelle and the German National Minorities of Europe, 1933–1945*. Chapel Hill: University of North Carolina Press. 1993. Pp. xiv, 335. \$39.95.

A relatively neglected aspect of the Nazi era is the fate of millions, those designated *Volksdeutsche*, who lived outside the boundaries of the Third Reich, mostly in

Eastern and Southeastern Europe. This gap has now been admirably filled by Valdis O. Lumans. Lumans makes no claim to examine all aspects of the prewar and wartime experiences of the German minorities, but his examination of how the Nazis’ *Volksdeutsche Mittelstelle* (Ethnic German Liaison Office), or VoMi, exploited these minorities to fulfill Hitler’s foreign policy ambitions and Himmler’s new racial order in the newly conquered *Lebensraum* is the “most comprehensive treatment of the subject to date” (p. 15).

During the prewar years VoMi was in charge of nazifying the German minorities and thereafter for making sure they cooperated with Hitler’s overall foreign policy goals. Although technically a branch of the Nazi Party, it eventually fell under Himmler’s domination and ultimately Hitler’s whenever the *Führer* chose to intervene in its affairs. Even though most members of the VoMi eventually joined the SS, somewhat surprisingly “very few seemed to fit the stereotype of the young, ruthless, ambitious careerist in the mode of Heydrich . . . [and] as a whole they were very well educated” (p. 55).

Lumans’s greatest contribution is showing how Hitler and the VoMi differentiated between German minorities in different countries. Some, such as those in the Sudetenland, were deliberately used to stir up difficulties for the host countries; others, like those in Poland, were used as excuses for aggression; still others, for example the Germans of southern Denmark, were supposed to serve as models of moderation for and cooperation with the Scandinavian people who were eventually to form part of Hitler’s Great-German Reich. To this end, one of the most important functions of VoMi was to restrain the emotions of the German minorities whose enthusiasm for the Third Reich and hopes for annexation to it led to embarrassing incidents with the potential for upsetting Hitler’s foreign policy ambitions at a time when he was not yet prepared for either diplomatic or military action.

Although Lumans cautiously concludes that the *Volksdeutsche* were probably even more attracted to National Socialism than the citizens of Germany, he notes that they were more frequently the victims than the beneficiaries of Hitler’s foreign policy and Himmler’s racial fantasies. German minorities in the South Tyrol, the Baltic states, and even more outlying regions of Eastern Europe desperately wanted to be annexed by the Reich. Instead, they learned to their bitter regret that they were to be physically removed from their ancestral homes in order for Hitler to cement relations with Benito Mussolini and Joseph Stalin.

Although the Third Reich and its instrument, the VoMi, was a disaster for most German minorities, a few did benefit. Although many Baltic Germans had refused Hitler’s call to return to the Reich in late 1939, by January 1941 those who remained in the now-annexed republics of the Soviet Union were desperately glad to have a second opportunity. Like-

wise, Germans in Ukraine were eager to escape the rampaging Red Army in 1943, although Himmler delayed their evacuation so long that their trek to Germany became an epic of misery. To its credit, the VoMi did its best, often under extremely difficult circumstances, to make life as comfortable as possible for the relocated Germans who were temporarily housed in VoMi camps.

This volume is handsomely produced by the University of North Carolina Press. It is accompanied by thirty-six pages of endnotes, a twenty-five page bibliography, and a detailed name and subject index as well as useful maps. Although the epilogue on the postwar fate of the German minorities is interesting, the book would have benefited from a summary and conclusion as well as by illustrations of some of the leading personalities and events. Nevertheless, it is well worth reading.

BRUCE F. PAULEY
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GÜNTER NELIBA. *Wilhelm Frick: Der Legalist des Unrechtsstaates; Eine politische Biographie.* (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1992. Pp. 418.

At Nuremberg, Nazi Interior Minister Wilhelm Frick said, "I wanted to attain everything by legal means. In essence, I am a jurist" (p. 371). His defense portrayed him as a legalistic bureaucrat, who lost out in the brutal internal party battles over organization and power until by World War II he exercised no political influence. In the tribunal's judgment, however, Frick belonged to the inner circle of the old Nazi elite, a powerful leader whose criminal activity on behalf of the Third Reich warranted the death penalty. In this well-researched political biography, Günter Neliba thoroughly documents that Frick's legal, legislative, and bureaucratic work for the Nazi Party did have far-reaching and fatal consequences. But Neliba's book offers much more, for it establishes that the driving force behind Frick's political engagement was a compelling desire to fulfill his intense racist and anti-Semitic convictions.

One of the oldest and most loyal of Adolf Hitler's close followers, Frick devoted about twenty-five years to the cause of National Socialism. From the early 1920s Frick used his skills and official positions in Munich to undermine the Weimar state, as well as to protect and advance the nascent Nazi movement; Hitler often said that had it not been for Frick he would still be in prison. Frick was, indeed, the model bureaucrat. A man of unquestionable administrative competence, Frick's self-image was of a public servant dedicated to the welfare of the German *Volk* and *Vaterland*. At crucial stages in Hitler's consolidation of power in 1933, and thereafter in the development of the Nazi state apparatus and its legislation, this "fatic of *Pflichterfüllung* [performance of duty]" (p.

379) was one of the architects of the Third Reich's governmental edifice.

But it was not the structure or practice of government he preferred. The dynamic and often arbitrary nature of the regime defied Frick's pursuit of law, order, and smooth, dependable bureaucratic regularity, especially since the governing principles of political decision making for Hitler were exceptional regulations and special powers. Frick's insistence on legality and bureaucratic efficiency was a political liability for him. Without Hitler's support Frick could not achieve the unity of the party and state, nor prevent Heinrich Himmler from usurping the police powers of the state. Still, the structures Frick established, and that he maintained as efficiently as possible under such conditions, constituted important requisite mechanisms for the regime's stabilization and the implementation of its domestic and foreign policies.

In his relentless attempts to fulfill Nazi racial ideological goals Frick was more in harmony with Hitler, although here too the interior minister always favored legal methods. Among Neliba's most important contributions is his demonstration of the continuity in Frick's racial, eugenic, and anti-Semitic views and activities from his work as a Reichstag deputy in 1924 through the Third Reich. Neliba provides an illuminating examination of Frick's role in Nazi racial purification, particularly regarding the purge of Jews from German society and the eugenic policies of sterilization and euthanasia.

Neliba's meticulously detailed coverage of some of these issues, however, reads like a document prepared for a legal indictment with paragraphs full of dates, document titles, and numbers. But overall this is a well-written political biography that provides new insights on Frick's life and politics. As the first comprehensive study of the subject, it fills a serious gap in previous scholarship.

JOSEPH W. BENDERSKY
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EDGAR WOLFRUM. *Französische Besatzungspolitik und deutsche Sozialdemokratie: Politische Neuansätze in der "vergessenen Zone" bis zur Bildung des Südweststaates 1945-1952.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 95.) Düsseldorf: Droste. 1991. Pp. 366. DM 78.

Until recently the regions under French occupation were indeed the "forgotten zone" of the thriving *Nachkriegsgeschichte*, or history of the founding period of post-1945 Germany. The French archives had been opening sluggishly, and evidently the small French Zone had a rather negligible impact on the West German state. There was a consensus that the French occupation had been the harshest in western Germany. It was only in the mid-1980s that revisionist-minded historians turned their attention to the

French Zone and began to paint a far more favorable picture.

Edgar Wolfrum's study is very much part of this revisionist wave. It does not, as the title might lead one to believe, cover the entire French Zone, but rather only the southwestern ministates of Württemberg-Hohenzollern and Süd-Baden. The central argument of the book is that, in contrast to the well-known mutual antipathy between the French and Kurt Schumacher's centralist and nationalist-sounding Social Democrats, a reformist French zonal administration and southern German Social Democrats with federalist sympathies found a natural affinity as both hoped to build a new German democracy through social and economic reforms. To argue this point, Wolfrum does not deny that the French military government often followed contradictory policies and practiced economic exploitation, such as dismantlement and deforestation, but he exposes and emphasizes another important part of the occupation, namely the enlightened and progressive efforts of the Administrateur Général Emile Laffon to build a model of a new democratic Germany in the French Zone. In contrast to the military governor General Pierre Koenig, Laffon, like High Commissioner Gilbert Grandval in the Saar, has for some time enjoyed a reputation as a "good Frenchman" in the occupation. Laffon in fact worked with labor unions and Social Democrats to democratize the economy and society through co-determination in the factories and mixed management-labor expert commissions (*Fachkommissionen*) and a Land economic council (*Landeswirtschaftsrat*) for economic planning and administration. Furthergoing efforts for socialization of industry and land reform ultimately fell victim to the merger with the Anglo-American bizon. In addition, Wolfrum might have cited the equally remarkable one-third labor representation in chambers of commerce, on which there is extensive documentation at the Wirtschaftsarchiv Baden-Württemberg—curiously missing among archives used in this well-researched book.

This perspective revises our image of postwar Germany in a small but important way. Clearly, the coverage and sympathies of the book are so heavily tilted toward the benevolent Laffon and the extraordinary Social Democratic statesman Carlo Schmid in Württemberg-Hohenzollern that one wonders whether the author's enthusiasm for "destroying legends" has not tipped the scales too far. No doubt the harshness of the French occupation weighed heavily on the population and seemed the all-encompassing feature of the occupation not only to German but also to American and Swiss officials of Quaker Relief. How far did the affinity of French administrators and Social Democrats go beyond inveterate Francophiles like Schmid or the South Baden separatist Friedrich Leibbrandt? Does Wolfrum's enthusiasm for the reformist achievements of the French zone betray too narrow a perspective on postwar Germany? Eco-

nomie councils and business-labor advisory commissions were quite typical of the other zones and generally emerged earlier with the support of American and British occupation officers, although anti-socialists came to predominate in the U.S. military government. But these are minor questions about an important book.

DIETHELM PROWE
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JAMES M. DIEHL. *The Thanks of the Fatherland: German Veterans after the Second World War*. Chapel Hill: University of North Carolina Press. 1993. Pp. xii, 345. \$49.95.

In this excellent study of the politics of social welfare legislation affecting German veterans of twentieth-century wars, James M. Diehl leaves no doubt that "in the area of veterans' politics . . . Bonn is definitely not Weimar" (p. 5). No one knows better than Diehl how antidemocratic veterans' groups and militaristic organizations undermined Weimar democracy; his book *Paramilitary Politics in Weimar Germany* (1977) did much to illuminate the nature of right-wing political mobilization against Germany's first democratic experiment. Diehl covers some of the same territory in the introductory chapters of this study, although his analysis expands to include a careful look at the state measures established in the 1920s to address the material and psychological needs of returning veterans of World War I. Despite the relatively generous programs implemented by Weimar governments, veterans' organizations were anything but grateful and remained virtually unanimous in their disdain for Weimar's democratic social welfare state. Attracted by Nazi assurances that in a militarized German society the "front experience" would be honored, veterans soon learned that "Like most promises of the National Socialists, those made to the veterans were specious" (p. 32); the Nazis did less for veterans than the "pacifist" governments of Weimar.

The Allied defeat of the Third Reich left Germany occupied by victors who were unified in including demilitarization along with denazification, decartelization, and democratization among the "variety of D's" that structured postwar occupation policy. Accordingly, the Allies in the western zones of occupation, Diehl's focus, prohibited veterans' organizations and cut off virtually all veterans' benefits and military pensions, which they dismissed as elements that had glorified the military and kept alive the "front ideology" of World War I. Still, disgruntled war veterans, war-disabled, and returning prisoners of war did not translate their anger and dismay over this treatment by the Allies into antidemocratic sentiment. Explaining why not is the central focus of Diehl's book.

Diehl argues that the West German state defused the potential radicalism of veterans by aggressively representing and addressing their interests and suc-

cessfully involving veterans in the formulation of programs to meet their needs. Although veterans never received everything they demanded—and the finance ministry relentlessly emphasized that the Federal Republic was an impoverished country in the throes of economic reconstruction from the ground up—the government of Konrad Adenauer quickly took action to introduce and implement a range of programs that assisted veterans and returning prisoners of war. West German policies were self-consciously formulated in response to the harsh actions taken by the Allies and the Allies' early postwar charge that all Germans were collectively responsible for National Socialism. Built on the foundations of Weimar social welfare legislation, the measures introduced by the first postwar government indicated West Germans' willingness to learn from the best of their own history and did much to justify the Federal Republic's claims to be able to run its own affairs without Allied oversight. "The laws represented an assertion of pride and independence—they were made in Germany, by Germans, according to German traditions" (p. 242). They provided compelling evidence that a democratic state could meet the demands of disadvantaged groups and effectively integrate one important sector of war victims into a new social and political order.

As Diehl rightly emphasizes, veterans were one among several groups of self-identified German victims of Hitler's war; others included civilians, bombed out of their homes, and expellees, who fled westward before the Red Army at the end of war and were driven out of Eastern Europe by communist governments after 1945. The blurring of the lines between war at the front and war at home, caused by extensive civilian casualties and the destruction of Germany's urban landscapes during the war, denied veterans any claims to a privileged knowledge of the horrors of modern warfare. Veterans after World War II largely accepted their status, not as heroes returning to a homefront that had remained immune to the devastation of the war as they had in 1918, but as victims of a common fate; they were tied much more securely to the rest of German society. Although acknowledging that the chorus of German war victims potentially drowned out the voices of others persecuted by the Nazis because of their race, religion, or political views, Diehl emphasizes that by addressing the most pressing demands of its own constituents, the West German state did much to stabilize the foundations of democratic politics.

Diehl's book offers a major contribution to the early history of the Federal Republic. Based on an exhaustive look at parliamentary, ministerial, and party political archives and the papers of the occupying forces and veterans' organizations, it provides the first systematic account of one of the most important chapters of postwar West German social welfare policy. Much more than the story of veterans' politics and veterans' organizations, it is a fascinating analysis

of how the West German state established its own credibility and created the framework that made possible a broad-based acceptance of a democratic political order.

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A. JAMES MCADAMS. *Germany Divided: From the Wall to Reunification*. (Princeton Studies in International History and Politics.) Princeton: Princeton University Press. 1993. Pp. xvii, 250. \$29.95.

DIRK PHILIPSEN. *We Were the People: Voices from East Germany's Revolutionary Autumn of 1989*. Durham, N.C.: Duke University Press. 1993. Pp. x, 417. Cloth \$49.95, paper \$19.95.

With the unexpected fall of the Berlin Wall in 1989 and the disappearance of the German Democratic Republic in 1990, we are fortunate that A. James McAdams and Dirk PhilipSEN were able, separately, to gather large numbers of interviews with participants, especially in East Germany, in these tumultuous events, before hindsight could alter the accuracy and detail of the memories of those interviewed. Whereas McAdams uses his interviews in both West and East Germany as incidental documentation to his wide evidence from newspapers, journals, secondary works, and government documents in his analysis of the relationship of West and East Germany from the Wall's erection in 1961 to unification in 1990, PhilipSEN seeks to throw light on the revolutionary autumn of 1989 in East Germany by allowing a wide variety of participants from many areas of society to speak for themselves of their part in, or their reaction to, the civic movement that brought about the collapse of one of the most rigid regimes in Eastern Europe.

McAdams notes that he is one of the political scientists who "are now turning away from purely deductive modes of political behavior to embrace the more traditional, historical study of political institutions" (p. xii). Working in this framework, he concentrates on the behavior of the political leadership in the two German states to show how the different systems of government affected not only the way in which decisions were made but also the decisions themselves. He reaches the paradoxical conclusion that the very difference of the two systems was a major factor in persuading the two governments to recognize their inability to change each other's character and, as a result, to accept whatever benefits they could by negotiating a *modus vivendi* in the Basic Treaty of 1972. He then draws a second, more telling paradox, arguing that all West German parties had by the 1980s become so convinced of the value of the new relationship that they could not pressure East Germany to make concessions to the emerging reform movement without endangering that relation-

ship. Therefore, they were unprepared to act in 1989 when the East German government was directly challenged by massive emigration and by enormous demonstrations at home.

The Wall as a determining factor in German-German relations is McAdams's central theme. Before it was built, he claims, there was a similarity in the position of West German Chancellor Konrad Adenauer and of East German Party Secretary Walter Ulbricht, in that each felt the need to demand that the superpower patronizing them give stronger support to their claim that a reunified Germany should represent their own political ideology whether liberal-democratic or socialist. In particular, Ulbricht feared that the Soviet Union was willing to recognize a capitalist West Germany in a reunified German state if it would stop West German rearmament. "In this single respect," McAdams remarks wryly, "West Germany's reentry into NATO may have been the best thing that could have happened to the East German government" (p. 27). President John F. Kennedy and Adenauer, he asserts, had also moved apart by the time the Berlin Wall was finished, since Kennedy's stated reluctance to do more than defend West Berlin alone was a crucial factor in persuading the Soviet Union and East Germany that the Wall could be erected with impunity.

McAdams describes in painstaking detail the elaboration of a new *Ostpolitik* by Chancellor Willy Brandt as a recognition of the permanence of the Wall and the disappointment in the late-1970s over its results. In the 1980s, he shows that Chancellor Helmut Kohl's government became locked into a policy that confirmed East German independence, but he deliberately refuses to speculate on why that independence was undermined from within. Because it concentrates on governmental policies and relations, McAdams's book will be interesting mostly to those seeking a well-documented synthesis equally balanced in its treatment of both East and West Germany.

Philipsen uses interviews conducted in early 1990, including a long and informative talk with the last Communist premier, Hans Modrow, to explain the collapse of the East German state. He first shows that the revolution of 1989 was not a sudden combustion but a movement that had been building for years in the work of such leaders as Frank Eigenfeld of New Forum and Harald Wagner of Democracy Now. He shows how the movement, as an example of "self-generated popular politics" (p. ix), was richly fragmented, with a wide and fruitful series of demands for change, but as a result was unlikely to become a unified political force later. After capturing the excitement of the months when the reformers triumphed, however, Philipsen sets out to make a case that the reformers had not intended the end of Communism to be immediately followed by the absorption of their country into West Germany on terms of inequality, and he even implies that the overwhelming Christian Democratic victory in the

East German elections of March 1990 may not have represented the true opinions of the voters.

Philipsen's sampling of people to interview is quite small, although the conversations are reported at length, and his preoccupation with the way "West German politicians and businesses bulldozed their way through the Wall and into the matrix of East German society" (p. 207) is not entirely convincing. But as an oral history his book makes a lasting record of the motives and reactions of unusual revolutionaries who may soon be forgotten.

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WILLIAM V. HUDON. *Marcello Cervini and Ecclesiastical Government in Tridentine Italy*. DeKalb: Northern Illinois University Press. 1992. Pp. ix, 261. \$32.00.

Marcello Cervini (1501–50) has long been a familiar figure to students of sixteenth-century church history, but William V. Hudon now offers us the first full-fledged modern biography of the man. Based largely on the seventy-volume *Carte cerviniane* in the Florentine archives, supplemented by other archival materials and an exhaustive search of the published literature, the book appears to be as thorough a study as diligent research can produce.

Hudon prefaces his work with an extended historiographical essay in which he explores past and current views of Catholic reform and Cervini's place in it. Here he raises the question central to the biography: were the ideological tensions that current scholarship emphasizes between accommodating reformers (*evangelisti*) and uncompromising reformers (*intransigenti*) really a significant factor in the course and direction of church reform, or have such categories ceased to be useful tools for inquiry? Further, does Cervini properly belong among the *intransigenti*? Hudon proposes to use his work on Cervini to offer us a more satisfactory understanding of the man and the early modern church.

Cervini, born to a Montepulciano family of substance with connections in Siena, Florence, and Rome, received a broad humanist education that, Hudon contends, shaped his views on church governance and goals and methods of church reform throughout his life. His career as a curialist began in 1534 with the election of Pope Paul III. First employed as tutor to papal grandson Cardinal Alessandro Farnese, Cervini was soon entrusted with increasingly important missions: legate to Charles V and Francis I, librarian of the Vatican, legate to the first sessions of the Council of Trent, and Inquisitor of the Holy Office. His rank advanced accordingly: cardinal in 1539; bishop-administrator in succession to Nicastro, Reggio Emilia, and Gubbio; protector of the orders of Servites and Augustinians. Cervini's enduring concern with reform at all levels of ecclesiastical

governance culminated with his participation in Julius III's reform commission of 1553–54 and his own preliminary efforts during the three weeks of his pontificate as Marcello II in April 1555.

Throughout the book, Hudon shows that Cervini remained on cordial terms with leaders of both the evangelist and intransigent camps. His views on reform were pragmatic rather than ideological, framed by his insistence on absolute papal authority and equally absolute episcopal authority on the diocesan level. Still, Hudon's conclusion that the ideological contest among reformers was irrelevant and might best be abandoned as a research model is not proven. In spite of Hudon's close perusal of the sources, the personality of Cervini remains elusive. Even his reform program emerges as vague, outlined only in the most general terms. Perhaps Cervini, the hard-working curialist, the loyal servant, the sincere reformer, the conscientious administrator, was neither evangelist nor intransigent, as Hudon convincingly argues, but that does not mean that the categories did not exist.

Finally, the book contains too many errors for a university press. Surely a vigilant editor would have noted, for example, that "ante passionem" (p. 205, n. 5) cannot be translated as "after the passion" (p. 73), or that the assassination of Pier Luigi Farnese, Paul III's son, took place in 1547 (p. 187, n. 41), not in 1549 (p. 27) and that therefore Cervini could not have continued corresponding with him "long after the death of Paul III" (p. 28). Despite some flaws, however, Hudon has presented us with a welcome addition to the literature on the Tridentine period. Unfortunately for the author, Cervini died too soon. Had Marcellus II been allotted his full span of years, his true self and his true reform plan might have been revealed. As it is, in spite of the most diligent research, the flesh-and-blood Cervini remains hidden.

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MARINA ROGGERO. *Insegnar lettere: Ricerche di storia dell'istruzione in età moderna*. (Forme e percorsi della storia, number 1.) Turin: Edizioni dell'Orso. 1992. Pp. xxv, 212. L. 25,000.

This volume of essays adds another feather to the cap of an accomplished Italian historian. Marina Roggero blends social and cultural history skillfully and probes deeply into the archival riches of pre-unification Italian states. Like her mentor, Giuseppe Ricuperati, she is interested in the role educational institutions played in the transformation of European society between the invention of movable type and the beginning of the Enlightenment. The history of pre-unification Italian society, whose leaders were torn between their allegiance to the pope and their desire

to secularize, provides ideal terrain for such investigations.

The volume's seven chapters were previously published as journal articles, but this does not detract from the usefulness of the work. The first two chapters deal with the rise and fall of Jesuit collegiate institutions for children of the Italian elite. Together these two chapters constitute a substantial short monograph on the educational philosophy of the Counter Reformation and the reasons why it declined by the end of the seventeenth century.

The Jesuit colleges accepted and provided scholarships for young men from impoverished noble families and the artisan classes. The highly structured Jesuit curriculum, the *Ratio studiorum*, provided a common classical foundation that was a prerequisite for the attainment of elite status. But equality of opportunity existed in theory, not in practice, because poor youngsters did not attend classes as regularly, stay in school as long, or graduate at the same rate as their more privileged classmates. And the Jesuit curriculum did not prepare students for useful employment. On the contrary, at the turn of the eighteenth century it produced scores of unemployed or underemployed clergymen, lawyers, and writers.

The third and fourth essays, which also complement each other, trace the development of the modern university in Italy. One major theme in that development was the struggle between students and municipal or state authorities for control of the colleges. Another theme was the influence, role, and aims of the benefactors who endowed the colleges.

The last three essays in the volume focus on one pre-unification state, Piedmont, from the Enlightenment to the 1830s. Roggero traces the growing influence of the monarchy and its attempts to establish centralized control over the educational system. Here she moves on well-trod historical ground. Into the well-documented social and political history of the last quarter of the eighteenth century she weaves fascinating new information about what was happening at the bottom of the educational system.

While the secular elite that replaced the discredited Jesuits focused their attention on the curriculum and teaching staff of the colleges, educational missionaries and entrepreneurs from all strata of society turned their efforts to teaching the alphabet to adult women and men and to the children of the urban poor and the peasantry. Progress was slow, the teachers often barely literate themselves, the facilities improvised. Roggero demonstrates convincingly that this voluntary, spontaneous, unofficial drive for literacy predated the French Revolution, although it was strengthened by it. And she argues that this movement, much more than the organized, conscious efforts of court reformers and enlightened clergy, prepared Piedmontese society for the age of agricultural reform, industrialization, and participation in a national culture based on a shared national language.

The subtitle of this volume—research on the his-

tory of education in the modern age—is a more accurate guide to its content than the title. *Insegnar lettere* is a clever, and I would guess intentional, double entendre. It refers both to the teaching of letters, that is the alphabet, and to the teaching of an Italian literary tradition, one of the building blocks for unification in the nineteenth century.

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MARC AGOSTINO. *Le Pape Pie XI et l'opinion* (1922–1939). (Collection de l'École française de Rome, number 150.) Rome: École Française de Rome. 1991. Pp. 820.

Pius XI (1922–39), often seen as the pope of missions, concordats, and social action, whose reign coincided with fascism in Italy prior to the outbreak of World War II, is presented in these pages as determined to re-Christianize contemporary society by influencing public opinion. This task, Marc Agostino indicates, colored the general orientation of his pontificate. Complementing the traditional mechanisms employed by the papacy to make its voice heard, Pius is perceived as an innovator in his use of radio, challenging Benito Mussolini's monopoly of the airwaves by inaugurating Vatican Radio. There were other innovations. His canonization homily for St. Thérèse of Lisieux (1925) was the first papal proclamation announced on a public-address system, and his encyclical *Viligante Cura* (1936) challenged the film industry to make the cinema an instrument of Christian instruction rather than pagan immorality. Above all, Pius is depicted as appreciating the impact of the newspaper press on the public, and Agostino's book examines the press reaction to Vatican developments during these critical years.

Although there are some 800 pages of text, Agostino does not pretend to provide an exhaustive, world-wide study of the papal impact on the press, focusing instead on France and Italy through the prism of a series of journals, reviews, specialized magazines, and studies. There are chapters on the Catholic press in France and Italy, the secular press in France, and the non-Catholic press in Italy. Included are a wide range of dailies, weeklies, and biweeklies, with a sprinkling of monthly magazines and quarterlies. Agostino does not attempt to provide a year-by-year analysis of papal events and actions, but concentrates on the highlights of the pontificate and the reactions they provoked. First and foremost, he examines the election and the death of the pontiff, which evoked the broadest commentary, expressing varied opinions and conclusions. Between his accession and death, there is a chronological assessment of the momentous events of almost two decades, including the reaction to the social encyclicals, the affair of the Action Française, the Lateran Accords, the crisis of Catholic Action in Italy, the Concordat with Nazi

Germany in 1933, the Ethiopian war, and the papal reaction to Italian and German totalitarianism and racism. Surprisingly, the Spanish Civil War is virtually ignored, and more might also have been said about the papal response to anti-Semitism in Germany. In part the coverage reflects the print media selected in France and Italy, mirroring its depth of historical vision and hindsight.

The introduction and the first three chapters provide useful information on the traditional means of papal expression, the general program of the pontificate, the organization and structure of the Italian and French episcopacy, and the official organs for the diffusion of papal information in the two countries. The conclusion offers an assessment of Pius XI's impact on the press, records the similarities and differences in France and Italy, and notes class, societal, and regional variations. In general, Agostino considers this pope's influence on the media important, especially in comparison to his predecessor, Benedict XV. He concludes that public opinion, especially in France, where it could be expressed freely, reacted more favorably to the papal message when Pius expressed the preaching mission of the universal church than when he sought to defend specific institutional prerogatives. His efforts on behalf of peace as well as such traditional values of Christian life as prayer, devotion, and the preservation of the family found resonance in large segments of French as well as Italian public opinion.

The volume concludes with a catalogue of the various French and Italian journals and reviews consulted, organized by country and category. This is followed by a broader bibliography listing sources on a number of topics, including many given short shrift in the text. Finally, there is an index of names. Although the reader will find little that is new in these pages, the study is a welcome addition to the historiography of a critical pontificate and a key figure in the formation of the modern papacy.

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JEAN-DOMINIQUE DURAND. *L'Église catholique dans la crise d'Italie* (1943–1948). (Collection de L'École française de Rome, number 148.) Rome: École Française de Rome. 1991. Pp. 879.

A topic of extraordinary complexity, the history of the Catholic church in the last years of fascism raises so many important issues—political, social, and religious—that even a *thèse* in the classic French tradition, with more than 700 pages of text plus 120 more listing sources, hardly seems too long. Jean-Dominique Durand accepts the ecclesiological perspective of his sources, combining a general account with a more detailed look at particular dioceses and the policies of individual priests. The intent in this work

of extraordinary learning is quite literally a history from below.

The information provided is remarkable, and others will mine it for years. There is a great deal on devotions, ordinations, religious orders, and diocesan structure. Pastoral letters and sermons, local clerical publications, and official statements are quoted at length, and we learn a good deal about clerical worries. These contain much that is revealing in itself and that is worthy of a kind of anthropological analysis, including the clergy's fear of Masonic influence and the energy they spent in familiar denunciations of the dangers of dancing, immodest dress, and the scandal of women riding bicycles. Such surprisingly strong echoes of the nineteenth century at a time of crisis, like much of the interesting material in this work, may well cast light on the relations between Catholicism and society. Paradoxically, however, Durand's institutional focus tends to heighten the sense of the church as an isolated institution preoccupied with its own concerns. Despite the richness of such details and the clarity with which they are set forth, exploring their significance is not the book's principal purpose.

Durand rightly insists on the church's centrality to Italian society in a time of painful transition, and he faces squarely the issue of the church's involvement with fascism and its impact on the establishment of the Italian republic. These are touchy matters, and the author strives for fairness. He finds it, as his ecclesiological perspective would suggest, in a careful reading of official statements and actions. We learn much less, therefore, of what Catholics did than what their leaders at many levels said, less about the church's effect on the torturous daily decisions Italians had to make than on what the clergy believed its role to be. That is interesting and important, but ultimately unsatisfactory for understanding the suspicions and hopes that revolved around the Catholic church from the fall of fascism to establishment of a republic dominated by Christian Democrats.

Within the limits he has chosen, Durand offers unparalleled information on Catholic organizations of every type, and his larger findings are ones all interested historians will need to take into account. Although acknowledging important exceptions, he insists that the bishops generally opposed the Republic of Saló and that official statements (by inference at least) justified resistance. He rejects the conventional emphasis on a difference of outlook between upper and lower clergy and is convincing on the moral concern with which they opposed reprisals on either side or the shooting of captives. He establishes the clergy's strong commitment to protecting the papacy and the vision they shared in the immediate postwar years of converting Italy into a Christian society. Having presented a great deal of specific information on the statements made and actions taken by Catholics in the postwar years, as in his discussion of the church's role in mobilizing for the elections of 1946,

Durand concludes that the church was neutral in the sense that it said it was. That neutrality, he insists, made the difference that permitted the establishment of the republic, for most members of the hierarchy were monarchists despite distaste for the House of Savoy, and the Christian Democrats could have saved the monarchy had they not been neutral.

With its wealth of information, admirable effort at even-handedness, and sensitivity to important issues at a crucial moment in Italian history, this study, like the sources it uses most, preserves the atmosphere of the view from the sacristy.

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ANTONIO CARDINI. *Tempi di ferro: "Il Mondo" e l'Italia del dopoguerra*. (Saggi, number 392.) Bologna: Mulino. 1992. Pp. 457. L. 50,000.

Antonio Cardini's interesting work details the influence of the weekly *Il Mondo* on the history of postwar Italy. Founded by journalist Mario Pannunzio, *Il Mondo* was published between 1949 and 1966. Most of the country's important liberal-democratic intellectuals wrote for the newspaper and, while its impact is often referred to in Italy, this is the first book that takes a comprehensive view of its enduring legacy. In the course of accomplishing this task, Cardini necessarily links the newspaper to the tortured political developments and intellectual battles of the postwar Italian world.

Essentially, the *Il Mondo* group championed Anglo-Saxon-style liberal democracy in Italy. Given Italy's circumstances, the intellectuals gathered around the weekly had no easy task. The *Risorgimento* had succeeded in linking the country to European liberal democracy in the nineteenth century, but Cardini rightly raises a question as to the depth of this commitment among the masses. Throughout the period preceding fascism, moreover, and particularly in the early years of the twentieth century, the liberal state faced serious challenges. Not only did parliamentary institutions suffer criticism, undermining its *raison d'être*, but the country witnessed the cultural triumph of irrational, nationalistic, and antidemocratic movements. Beginning in the 1920s, fascism cut Italian ties to European liberal democracy, and by the 1930s communism, Nazism, and fascism seemed to have dealt a death blow to the entire liberal democratic movement. The United States and Britain were able to reverse this trend beginning only in 1939–41 thanks to their superior military prowess.

In Italy, however, the links had been severed to such a degree that "we can ask ourselves if the Italy of 1945 was not so weakly oriented [in the liberal-democratic direction] as to be unable to give expression to political forces which could identify completely with Europe and the United States" (p. 19). In

the 1946 elections the programs and ideology of the parties that won practically all the votes were only superficially similar to prevailing liberal democracy in the United States and Britain, while the masses did not sympathize with that political ideology as a goal. In fact, the Communists hoped to install a regime on the Soviet model while the Catholics considered their "Christian corporatism" a "third road" between capitalism and communism (p. 281).

Enter *Il Mondo*, which attempted to construct an Italian liberal-democratic ethos. The group around this publication was inspired by the Anglo-Saxon model that they argued had built a system that had brought the best life to the greatest number of people. Despite this, the major reference points were Italian: Gaetano Salvemini, Benedetto Croce, and Luigi Einaudi, whose principles formed a hard-core liberalism Benito Mussolini had found impossible to extirpate. During the 1950s and 1960s, *Il Mondo's* writers pushed the nation toward the Western model, defending democracy and condemning the fascist protectionist legacy. It was also during these crucial years that Italy changed from a country of peasants into a modern industrialized country. Cardini's book chronicles the cultural debates accompanying this development until the 1960s. That decade witnessed the rise of the New Left in the United States and Europe, and for *Il Mondo* the disappearance of the Anglo-Saxon liberal-democratic reference point. In Italy, the reform era, disappointing as it was, gave way to the *contestazione*, a change *Il Mondo* could not survive.

On March 8, 1966, *Il Mondo* ceased publication. Pannunzio's comment on the passing of his weekly at the same time explained why its job was not finished and why its long struggle had ended: "the pressure of enormous masses who vote for the Catholics, for the Communists, and even for the monarchists and the Fascists imposes, through sheer numbers, ideals and political, cultural, and moral conceptions . . . which are far from those of the modern world" (p. 436). Italian liberals (in the American sense) remained cut off from the fabric of the nation.

Cardini explains the essential failure of the group by stating that cultural initiatives of the kind undertaken by *Il Mondo* follow and do not precede economic transformations. Italy was modernized during the period under discussion but the cultural effort failed to keep pace. Only the Cold War's end, Cardini concludes, has established the basis for harmonization of a modern cultural and economic structure. That remains to be seen.

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JOHN P. SPIELMAN. *The City and the Crown: Vienna and the Imperial Court 1600–1740*. West Lafayette, Ind.: Purdue University Press. 1993. Pp. ix, 264. \$32.00.

When the Babenberger, a notoriously peripatetic ruling house even by medieval standards, first chose Vienna as their seat, it was little more than a village. So it remained until the advent of the House of Habsburg in the mid-fourteenth century, when, profiting from the decline of Czech influence in the upper Danube basin, the city began to flourish. Yet the general economic decline of Central Europe in the fifteenth century soon ended this brief prosperity. To this decline must be added the circumstance that, in the Protestant Reformation, the city's burgher elite wound up on the losing side and was duly punished for its effrontery by an increasingly Catholic monarchy. A post-Reformation economic recovery was cut short by the general European depression of the seventeenth century, which, in Austria, was exacerbated by the economic consequences of the Thirty Years' War, a number of outbreaks of the plague, and the depredations of the Ottomans.

The point of this melancholy recital is that the Habsburgs, building on the territorial gains that luck and clever dynastic maneuvers had brought them, and having established a quasi-absolutist monarchy on the French and Spanish models, found themselves residing not in a major, prosperous, and dynamic commercial center, but rather in a city wholly dependent on them. Vienna was, in fact, a *Residenzstadt*, not unlike the capitals of so many minor German princes, only somewhat larger.

From the foregoing premise, John P. Spielman proceeds to unravel the complicated relations between the court and the town. Making good use of municipal records, long neglected by historians of the monarchy, he builds a plausible and mostly convincing picture of life in the Habsburg capital. In particular, his use of the relatively complete records of the department in charge of quartering both troops and court personnel on the citizenry is ingenious.

The story that emerges, although it unfolds as a theme with variations, is essentially simple: the crown was consistently short of funds; the needs of the city, whether for defense against foreign invaders, recovering from natural catastrophes, or providing social services, almost always exceeded what it was capable of contributing in tax money under existing dispensations. As a result, the crown habitually squeezed whatever it could out of the burghers in other ways. These actions took various forms: arbitrary assessments; actual confiscations, as was the case in 1670 when Leopold I expelled Vienna's Jews; and, above all, the quartering system, which saved the crown huge sums of money that otherwise would have had to be expended for the construction and maintenance of barracks and residences for the court. Furthermore, large amounts of money could be raised by selling immunities from the highly unpopular quartering system.

The result of all this was that, instead of proceeding *pari passu* with the expansion of the monarchy, Vienna's economic growth tended to develop inversely to

it; the more powerful the monarchy, the greater its arbitrary incursions into the city's economic life. Over time, this condition could not be other than counter-productive. In order to make themselves into a power of the first rank the Habsburgs would have needed to transform their capital into a major financial and commercial center, a transformation prevented by precisely the policies that they habitually espoused. Obviously, there were other and broader factors hampering Vienna's economic growth, but the short-sighted policies of the ruling house should not be overlooked.

Spielman has given us an important study that opens up new vistas into pre-Theresian Austrian history. The book is not, as he himself is the first to point out, a comprehensive social history of Vienna in the early modern period (we must await the comprehensive sifting of the voluminous and complex municipal records), and it is more convincing for the seventeenth century than for the eighteenth century. But it is a soundly reasoned, engagingly written, and an intellectually stimulating work.

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HARRIET ANDERSON. *Utopian Feminism: Women's Movements in fin-de-siècle Vienna*. New Haven: Yale University Press. 1992. Pp. xiii, 322. \$35.00.

The simultaneously fecund and repressive society of *fin-de-siècle* Vienna has an inherent magnetism. Thus, we have high hopes for a study of the aspirations and goals of "progressive" (p. x) women living in this turbulent milieu.

Harriet Anderson's thesis is that progressive Viennese women—"feminists" would be our term—did not limit their interests to practical goals. Although the feminists did indeed campaign for such basic rights for women as state-supported secondary and higher education, vocational opportunities, and the vote, their aim was far broader. Enhancing women's lives, the feminists believed, would lead to a new social order where both men and women could lead richer lives. A women's movement therefore would be a "moral force [for] humanity" (p. 10), leading to "the upward development of the human race" (p. 20).

In addition to presenting evidence for this thesis, Anderson also challenges some of the recent literature on Austrian feminism, notably the work of Richard Evans, Barbara Gutt, Ingrid Lafleur, and Herrad Schenk. Moreover, Anderson calls on us to reevaluate the view of *fin-de-siècle* Vienna as an apocalyptic and decadent society. There was a "flourishing culture" (p. 253) of social reformers, including feminists, who believed in the possibility of a new age of freedom and love devoid of misery and violence.

Anderson develops her primary thesis in three distinct sections. In the first, we are introduced to

various women's organizations, the one most representative of the feminists—although not at all the largest—being the General Austrian Women's Association. We meet feminist leaders Auguste Fickert, Rosa Mayreder, Marianne Hainisch, Marie Lang, and Grete Meisel-Hess and learn of both the immediate and visionary reforms they sought. It is worth noting that suffrage was far from being the central issue for the feminists. Perhaps this was due to the bleak fact that women were forbidden to belong to political parties. That is why they founded in 1893 a "general association" rather than a political association.

In the book's second section Anderson lays before us the feminists' theoretical writings. Like their organizations, the women's analyses and the reforms they urged had the higher goal of a healthier and more just society for all. Anderson attempts to show the relationship of their work to some of the prominent intellectual currents of the nineteenth century, such as liberalism, neo-Romanticism, Darwinism, and socialism.

Finally, Anderson's section on the feminists' fictional writings treats many of the same writers whose theoretical works were previously examined. Their themes are what one would expect: love, romance, prostitution, rape, the double-standard, suffering under the prevalent moral codes, the problems of working women, marriage, motherhood. An interesting aspect of this section is what Anderson dubs "psychological feminism" (p. 228): Meisel-Hess and Mayreder's attention to the role of the unconscious and their cutting-edge style, with interior monologues and an impressionist, associative diction. Anderson concludes that the feminists' fiction was not as crusading as their theoretical efforts, but she defends their work as "realism" (p. 245).

"In the last resort," Anderson decides, "[the feminists] failed" to bring about the "deeper changes [they] envisaged" (pp. 250–51), underestimating the strength of the ideals of the nuclear family and motherhood as well as the resistances of men to change. This, by the way, is the only time when Anderson is even mildly critical of the Austrian feminists. A keener eye throughout would have been welcome.

Whether viewed conventionally or from Anderson's perspective, Vienna at the turn of the century was a compelling and vivid place. Yet the life of the city and the feminists' interaction with it are precisely what Anderson fails to capture. Her style is plodding and repetitive, and the sections on the feminists' theoretical and imaginative writings in particular lack grace. Her book is, however, a reference to which scholars can turn for information, in English, on Austrian feminism before World War I.

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DONALD E. MILLER and LORNA TOURYAN MILLER.
Survivors: An Oral History of the Armenian Genocide.

Berkeley and Los Angeles: University of California Press. 1993. Pp. xii, 242. \$25.00.

This book is a history of Armenian-Turkish relations during World War I. There have developed in the historical literature two opposing views of those relations, one asserting the existence of a genocide of the Armenians, the other asserting a war of mutual destruction between Armenians and Muslims. This study takes the former view. Donald E. Miller and Lorna Touryan Miller draw extensively on interviews with Armenians who lived at the time and include an analysis of the psychological factors found in the interviews.

The authors understand some of the methodological difficulties in their study. The primary question is the reliability of evidence taken sixty or seventy years after the fact from witnesses who "were, on average, eleven or twelve years old in 1915" (p. 31). The Millers take such criticism in stride, declaring that the obvious emotional commitment of the survivors indicates that their stories have been believed firmly all their lives. This is undoubtedly correct, but it begs the real question of whether or not what they believed is wholly true. Recent studies of children's testimony in child-abuse cases in America bring up disturbing questions of the reliability of children's testimony. Nevertheless, despite possible embellishments, it is obvious that these children suffered horrors.

The methodological problem that the authors do not consider is the inherently biased nature of such interviews. Children may remember evils done to their families, but they are unlikely to remember evils done by their families. Indeed, it is unlikely that they would have even been told. Can one imagine a report of someone who was eleven years old in 1915 in which the survivor states, "My father went out and killed a number of the enemy and their families, then the enemy came and killed him and my mother"? Of course not. What a child of 1915 would remember was the sadness and terror in his or her own life. This would be true whether the interviewee was Armenian, Turkish, Kurdish, or from any other people who have suffered. In fact, comparable interviews of Muslim survivors of the war with the Armenians, some made in 1915-16, some made in the 1980s, tell of Armenians killing Turks and Kurds, not of Turks and Kurds killing Armenians.

The inherent difficulty in interviews such as these is that they only tell one side of a complicated story. Armenian suffering is described in detail. Turkish and Kurdish suffering goes unmentioned. Interviews of Armenian survivors, as well as of Turkish and Kurdish survivors, should have a place in the writing of the history of Anatolia during World War I. Their proper place should be as part of an even-handed history of events. The authors do not write such a history. Only one scholarly book that disagrees with their conclusions is included in their bibliography, and its conclusions on the events of World War I are

not referred to. One other such article is cited in the notes. The history in the book comes exclusively from sources that agree with the authors' preconceptions. This is carried to an absurd extent: the sections on the religious communities (*millets*), sultan Abdülhamid II, the Ottoman economy, the Ottoman portion of World War I, and so on, draw on none of the standard histories. Not a single book in Turkish, nor even a book written by a Turk, is included in the bibliography. None of the books that present an alternate view of the history of Armenian-Turkish relations have been consulted.

This is a book that harkens back to the "national histories" of a past age, histories in which nothing is entertained that might contend with the national vision. Opposing views are avoided. Today it is considered a better practice for historians to at least refer readers to books and articles with alternative views. That is one of the practices that separates history from propaganda.

JUSTIN MCCARTHY
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ALEXIS ALEXANDRIS. *The Greek Minority of Istanbul and Greek-Turkish Relations 1918-1974*. (Bibliotheca Asiae Minoris Historica, number 1.) 2d ed. Athens: Centre for Asia Minor Studies. 1992. Pp. 380.

In the aftermath of the Greek-Turkish conflict a compulsory exchange of populations led to the elimination of the age-old Greek communities in Anatolia. By the Treaty of Lausanne, however, the Greek Orthodox established in Istanbul were allowed to remain and were accorded rights as a non-Muslim minority. They were to maintain numerical proportionality with a Muslim-Turkish minority in Greece residing in western Thrace.

During the interwar era the Greek Orthodox minority of Istanbul numbered more than 100,000 people. In the *Economist Atlas of the New Europe* (1992), however, the country profiles note that Greece has minorities but for Turkey there is no mention of the presence of such groups. Alexis Alexandris's account documents and explains how this situation has come to be.

As the title of the book makes clear, this is not so much a study of the inner dynamics of an ethnic group, but of the politics—national and international—surrounding, relating to, and influencing the existence of a national community. Although the book is noted as a second edition, it actually is a reprint with typographical errors corrected.

The account of the Greek minority in Istanbul focuses on two interrelated worlds: the ecumenical patriarchate and the Greek Orthodox community. It is largely institutional history, with chapters on the patriarchate and its relations with the Turkish government alternating with others on the status and condition of the Greek community.

An introductory chapter surveys the Greeks and their institutions in the Ottoman capital from the late nineteenth century to the beginning of World War I. The story then picks up with the defeat of the empire and the rise of the Turkish nationalist movement. The negotiations at Lausanne pertaining to the establishment of the Greek minority in Istanbul and the issues dealing with minorities in the treaty are discussed. The chapters that follow cover the vicissitudes of the Greek minority in Istanbul decade by decade.

The thread that runs through the work is the dynamics of a triangular, interactive relationship involving the Greek minority, the Turkish state, and the Greek government. The fate of the Greeks and the ecumenical patriarchate became tied to the state of relations between Turkey and Greece in the context of significant international developments such as the Great Depression, World War II, and the Cold War.

Not unexpectedly, Alexandris finds that the fortunes of the Greek minority improved or fell in barometric synchronization with the fluctuations in Greek-Turkish relations. The author explains clearly the new development in power relationships from the days of the Ottoman empire to the successor Turkish national state. During the nineteenth century, the great powers of Europe were able to intervene on behalf of a nationality in the shrinking Ottoman domains. The Turkish nationalists, with Mustafa Kemal Atatürk leading the way, were determined not to allow such outside intervention to occur. As much as Greece might try to see that the rights of the Greek minority were upheld, its ability to influence the Turkish government was limited.

Over the half century that essentially saw a progressive deterioration in the numerical and material condition of the Greek minority in Turkey, two factors played a role in ameliorating the situation, at least for a while. One was personal. Charismatic national leaders such as Eleftherios Venizelos and Mustafa Kemal Atatürk brought about a rapprochement between the two states in the early 1930s. The second, impersonal factor was the division of the world into ideological and military power blocs. Cold War necessities also brought the two neighboring states together briefly in the late 1940s and early 1950s.

Yet there was a constant in the up-and-down course of Greek-Turkish relations. Turkey's political leaders were determined to break with elements of the past and create a new society on the basis of two seemingly progressive policies: secularism and national integration. For people like the Greeks who had survived through ethnic distinctiveness based on faith and communal organization, there was little room for secure accommodation. Their future in the Turkish national state promised little choice: either assimilation or the gradual elimination of ethnic diversity.

Alexandris provides a balanced and well-documented account of an important aspect of the often

difficult relationship between two nations that are joined by geography and history.

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DAVID M. CROWE. *The Baltic States and the Great Powers: Foreign Relations, 1938–1940*. Boulder, Colo.: Westview. 1993. Pp. xv, 264. \$55.00.

The Molotov-Ribbentrop pact of August 23, 1939, was officially a non-aggression pact, but it secretly divided up Eastern Europe between Nazi Germany and the Soviet Union. Because of the controversies that have surrounded this treaty, it has engendered an enormous literature. Or perhaps one should speak of at least three literatures: the background and nature of the pact itself, the disputes among historians about the nature and the very existence of its secret addenda, and its role in the last years of the Soviet Union when it even found a place on the agenda of the Soviet Congress of People's Deputies in 1989. The collapse of the Soviet Union brought no end to these discussions; there will be more to come.

David M. Crowe's study of the role of the Baltic states of Latvia, Lithuania, and Estonia in the politics of Eastern Europe can serve as something of a summa of the Western literature on the background and nature of the pact. The documentation is impressive: footnotes constitute almost 25 percent of the volume; the bibliography covers thirteen pages. Crowe has used published diplomatic documents as well as records in the British Foreign Office and in the U.S. National Archives. Those making further efforts to consider this aspect of the pact's history would be well advised to begin with this work.

The book's particular strong point is its comprehensive survey of the problems with which diplomats and politicians had to deal in the last years before World War II. Crowe considers topics often ignored by others who deal with this period, such as the repatriation of Germans from the Baltic republics in 1939 and 1940 and the tension between Lithuania and Poland over the Lithuanians' acceptance of the city of Vilnius (Wilno) from the hands of the Soviet government.

The picture that emerges from these pages is of the two large neighbors, the Soviet Union and Germany, closing pincers on the small Baltic republics. The availability of German documents allows Crowe to make clear the cynicism involved in protesting the rights of German minorities in the Baltic and challenging statements by Baltic leaders that might somehow question the Germans' motives. The absence of Soviet archival sources makes the Soviet leaders appear somewhat more speculative and problematic, although hardly less cynical.

But here some questions arise. The book might better have its title reversed—The Great Powers and the Baltic States—because the Baltic republics appear

more as victims, as objects, than as actors. Crowe recounts how the Baltic leaders kept trying to put the best face on their retreats, but he does not probe into what they must really have been thinking. Or did they really believe everything that they were saying? The German and the Soviet spokespersons did not necessarily believe their own words; the Baltic leaders must have had other thoughts than what they expressed in public for the benefit of foreign ears. In this respect the book fits too easily into the spirit of "comparative victimology" that tends to dominate the historiography of East European nations.

Despite Crowe's prodigious research, there are other resources available in the West that could have contributed to this work. Émigré Polish archives at the Hoover Institution at Stanford University, for example, contain rich materials concerning the Polish attitude toward the Lithuanians after the Soviets' seizure and cession of Vilnius as well as other materials concerning this period. The Edvardas Turauskas archive at the Hoover Institution has a wealth of information on Lithuanian policy and politics in 1938 and 1940. Subsequent efforts to build on Crowe's work must take these sources into consideration.

The role of the Molotov-Ribbentrop pact in the last days of the Soviet empire brought historians in the Baltic to the topic, looking for materials for their representatives to use in Moscow and also for their own needs. At the same time, the Western researcher must remember that Belarus, Ukraine, Moldova, and Poland also have interests in studying the pact. The topic in all its aspects is probably already too big for just one historian to tackle. Crowe has made a positive contribution to the historiographic tangle by producing a summation of the context into which the pact fits.

ALFRED ERICH SENN
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RICHARD BLANKE. *Orphans of Versailles: The Germans in Western Poland, 1918-1939*. Lexington: University Press of Kentucky. 1993. Pp. xii, 316. \$39.00.

The German minority in the territories ceded to Poland after World War I has been studied mainly as a function of German-Polish relations, a pawn in Germany's revisionist foreign policy, and a measure of the effectiveness of the League of Nations in resolving international disputes. This book by Richard Blanke is the first to focus on the minority itself, to describe and analyze its plight and its dilemmas.

The German minority in western Poland, estimated to number some 830,000 following a major exodus in the first years after the war, constituted a substantial part of the landowning agricultural population in Poznań and Pomorze and a dominant element in the more urban and industrial region of Upper Silesia. These Germans did not accept their new

status—or the existence of the new Polish state for that matter—as permanent and saw themselves by and large as Germany's "lost children, waiting to be reclaimed" (p. 54). Under the circumstances, even the most accommodating host country would have found this minority a challenge. Poland, which regarded Germany's refusal to accept its postwar frontiers and Germany's vocal support of the minority as threats to its newly restored sovereignty, was not accommodating. Although constrained by the terms of a minority protection treaty administered by the League of Nations, the Polish government, backed by public opinion, took steps to reduce the minority population and its influence. These policies, particularly intensive after 1926, were directed at the minority's political role, its schools and churches, and its landholdings and industrial enterprises. In this unequal contest, the German minority turned to Germany for support.

Blanke recounts the minority's attempts to maintain and improve its situation. He describes in detail the options at hand—the forms of adjustment available in the new environment, whether cultural autonomy, coexistence, or assimilation; the search, largely frustrated, for concessions and workable solutions; the intervention of various international organizations—and he concludes that Germany's political and financial assistance was the only way the minority could survive. Growing dependence on such assistance made the minority subservient to German policy, foreclosing any possibility of coming to terms with the Polish majority. Blanke shows why National Socialism so greatly attracted the Germans in western Poland and traces how German support and the minority's acceptance of German support changed. During the period of German-Polish rapprochement, the German government enjoined the minority to reduce its customary hostility to Poland and then, as German-Polish relations deteriorated, abandoned it to increasingly violent attacks from Polish chauvinist groups. Finally, the minority and its sufferings ultimately had become no more than a useful pretext when Adolf Hitler launched his war on Poland in 1939.

This book opens a window on a neglected subject. It is remarkably balanced, given the nature of the topic, which, until recently, was highly charged. Blanke takes issue with those who have justified Poland's de-Germanization policies on the grounds that the development of national cohesiveness required the reduction of German influence in western Poland. Interwar Polish nationality policy, he points out, never considered possibilities for integrating this minority into Polish national life. He is no less critical of those who have interpreted Germany's support of the German minority as little more than cynical manipulation on behalf of a revisionist foreign policy. No German government, he argues strongly, could remain indifferent to the fate of its former citizens (nor to the difficulty of eventually having to absorb large numbers of emigrants from Poland); thus the

question was never whether, but rather how, to extend assistance. An assessment of the different strands of German policy, some more flexible than others, and of the evidence available on Germany's support for German minorities in other "lost" territories might modify that view. In fact, neither German foreign policy nor German political opinion was as undifferentiated as it might appear here.

CHRISTOPH M. KIMMICH
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DAVID ENGEL. *Facing a Holocaust: The Polish Government-in-Exile and the Jews, 1943-1945*. Chapel Hill: University of North Carolina Press. 1993. Pp. x, 317. \$42.50.

David Engel's book deals with one of the more sensitive aspects of the Holocaust. The actual implementation of the Nazi genocide of European Jewry took place on Polish soil and its single largest group of victims were Polish Jews, who were, formally at least, Polish citizens. The attitude and actions of the legitimate Polish government, then in London, were of great importance. Although no misunderstandings should be fostered as to the ultimate Nazi responsibility for the Holocaust, the dimensions and the ability of the Nazis to carry out their plans were affected by the policies, attitudes, behavior, and active cooperation of the non-German peoples. It certainly is not a simple equation, however, for there existed a rough correlation between the willingness of the local populations to cooperate with the Nazis in exterminating the Jews and the levels of "success" their plans achieved in the different European countries. Denmark, Bulgaria, and Italy, for example, where the local authorities and populations refused to a large extent to collaborate with the destruction of the local Jewish communities, had relatively higher rates of Jewish survival than countries such as Lithuania, Latvia, and Estonia, whose populations were eager to collaborate. Thus, the policy of the Polish government becomes crucial for obvious reasons.

Engel starts this book where he concluded his earlier study, *In the Shadow of Auschwitz* (1987). He traces the evolution of the Polish policy in the second half of the war, when Nazi designs became obvious and well known. Engel relates in great detail the story of the relations between the government in exile, its representatives and diplomats all over the world, and different leaders and officials of the many Jewish organizations active at the time. The interests and concerns of the two sides were only partially compatible. The Polish government was mainly interested in re-creating the Polish republic in its pre-September 1939 borders. Defeating Germany and canceling the Soviet gains of Polish territories in the wake of the Molotov-Ribbentrop agreement were the major Polish preoccupations. They tried to recruit any assistance to convince the Western Allies to support

Poland's territorial claims, in the final count, with little avail. The Jewish organizations and influence were considered of some value in the Polish struggle. Yet, as becomes abundantly clear from the study, the Polish government in exile did not consider it important enough to offer real assistance in rescuing Jews facing annihilation in Poland. The activity of the Polish government in exile, in the face of the genocide that was taking place in Poland, was glaringly meager. It included some diplomatic activity with the Western Allies: the establishment of a powerless and largely inactive governmental rescue council, and a belated and half-hearted call to the Polish underground to offer assistance. Engel maintains that this inaction corresponded to the basic attitude of the Polish government that considered Jews as tolerated citizens, certainly not equal to the ethnic Polish population. The government did not feel itself duty bound to its Jewish citizens. It should be added that one could hardly expect a different policy from a Polish government that represented the Polish political elite of the Second Republic. It was the same elite that on the eve of the war created in Poland the most outrageous manifestations of anti-Semitism, including legal discrimination, boycotts, pogroms, and plans for mass expulsion. The deeper motives and explanations for the behavior of the Polish government while "facing a Holocaust" can, at best, only partially be found in diplomatic notes, government memoranda, minutes of meetings, and conversations, the major sources Engel uses. They have to be looked for in much deeper recesses of the cultural and mental make-up of the participants, and in Polish-Jewish relations.

Engel's study contributes considerably to our knowledge of the Polish policy. He uses wisely an impressive variety of archival materials and secondary literature. A more rigorous editing would have improved the text. This volume, although it carries a different title, relies heavily on the previous book and does not stand independently. A good editor would have found a way to combine the two books into one volume mainly by cutting out some lengthy and detailed discussions of minor issues, as well as much of the redundant and frequent apologies, in an attempt for "impartiality." (The statement about "the book's morally neutral character" [p. 12] as a goal is just one unfortunate assertion by the author to prove scholarly impartiality.) The evidence and Engel's intelligent treatment of his sources are sufficient to establish the scholarly integrity of his exhaustive study. There is no need for any apologies.

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CATHY A. FRIERSON. *Peasant Icons: Representations of Rural People in Late Nineteenth-Century Russia*. New York: Oxford University Press. 1993. Pp. x, 248. Cloth \$35.00, paper \$17.95.

As Cathy A. Frierson tells the story in this book, at the time of the Emancipation in 1861, educated Russians knew little about peasants but had great expectations. They believed that the peasants were the repository of Russian national character and that the liberated village held the key to Russia's future development. They further believed that peasant virtue was founded on moral communalism. Some who went to the village to learn more about peasants firsthand found virtuous communalists, but most did not. Some salvaged the image of communalism and mutual assistance, but only by linking it to subsistence farming: communalism was a defensive adaptation to hardship rather than an expression of a unique national character. That sort of communalism, moreover, was vulnerable to economic change: as money penetrated the village, peasants began to exhibit selfish individualism and a propensity to exploit each other; those who abandoned subsistence agriculture lost their moral bearings; peasant women, the educated outsiders believed, were especially likely to pursue selfish ends, and they also instigated the breakup of the extended family, a development that threatened both peasant patriarchy and peasant well-being. Peasant women as the victims of vicious beatings also served as the emblem of the violent depravity among peasants that educated observers found so disheartening. By the late 1880s, educated Russians interpreted their estrangement from peasants differently than they had in the 1860s: peasants were no longer the seat of virtue who could redeem Russia, they were instead vicious degenerates. The educated thereupon turned their attention to more promising social groups.

Frierson has, probably, accurately plotted the discussion of peasants on the pages of the principal intellectual journals of the 1860s through the 1880s, but both the opening scene and denouement are unconvincing. In her telling, the only ideas about peasants that educated Russians had at their disposal as of 1861 were highly romanticized images produced by Alexander Radishchev and Nikolai Karamzin, and the ethereal musings of the Slavophiles. She does not mention even in passing the major folkloric and ethnographic materials published before 1860; these too were part of the discourse on peasants, as were the numerous amateur ethnographic reports that appeared in the provincial press from the 1840s on. Furthermore, if the thick journals turned their attention elsewhere in the 1890s, there was another outpouring of ethnographic description, and organized collection of ethnographic data in that very decade and into the twentieth century; the search for the essential peasant continued, even if in a different forum. Images that were based on the most naive assumptions of innate peasant morality, communalism, and patriarchalism—images that Frierson asserts had been shattered by confrontation with village reality—persisted up to the 1917 revolution, and beyond. Sentimental monarchists, many Social Revo-

lutionaries, and Aleksandr Chaianov and his school certainly did not subscribe to the bleaker conclusions about ineluctable peasant degradation.

It is a shame that Frierson did not extend her analysis backward and forward in time, because what she has to say about the writings of the 1860s to the 1880s is quite perceptive. At the very least, she has provided a first-rate synopsis and critique of the major (and many minor) statements on the character of the Russian peasant. Her explication of Aleksandr Engelgardt's views on the economically rational peasant and the logic of mutual assistance is particularly good, and she makes clear why Engelgardt's writings had so profound an impact on his contemporaries. Her discussion of the competing views on peasant customary law—and of the intellectual stakes involved—is equally acute. So is her examination of the pervasive hostility to peasant women. And these are only a few of the high points.

Frierson does more than provide well-reasoned summaries; she also offers an explanation of why educated Russians reacted as they did to what they saw in the village. Their almost universal hostility to the involvement of peasants in a money economy and to kulaks was rooted in their own antipathy to competitive individualism, Frierson argues; peasants who did not live in an archaic world of economic self-sufficiency did not meet the intelligentsia's intellectual needs. The educated could not overcome their habit of viewing peasants as a collectivity rather than as individuals. Nor could they shed the notion that peasants were fundamentally helpless children in need of instruction and protection. When peasants did not take the hand extended to them, intellectuals concluded that peasants were incapable of salvation.

Although Frierson's analysis of the motives of her writers is not always completely developed, she makes a persuasive case that when Engelgardt, Gleb Uspenskii, Aleksandra Efimenko, and others wrote about peasants they were revealing themselves as much as their ostensible subjects. The conclusion to be drawn from her study is that even the most valuable accounts of peasants, such as Engelgardt's, should be approached first as documents of intellectual history, and only cautiously and secondarily as sources on how Russian peasants really lived.

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MARIA CARLSON. *"No Religion Higher than Truth": A History of the Theosophical Movement in Russia, 1876–1922*. Princeton: Princeton University Press. 1993. Pp. vi, 298. \$37.50.

Theosophy was a movement that was originated by a Russian, Mme. Blavatsky (born Elena Gan). Together with Henry Olcott, she founded a Theosophical Society in New York in 1875, and four years later they established a world headquarters for the group in

Madras, India, where it remains. She died in 1891, and her successors as leader were Olcott (d. 1907) and Annie Besant (d. 1933). Rudolf Steiner, an Austrian occultist thinker, joined the society in 1902 but defected in 1913 to form an anthroposophical society. A Russian Theosophical Society was then chartered in 1908 and led by Anna Kamenskaia for the next decade. The Bolsheviks suppressed the movement, but Kamenskaia managed to reconstitute the society outside Russia in 1926. She died in Geneva in 1952. Maria Carlson offers an account of these developments and analyzes theosophical doctrines and the differences that arose among their adepts. Her scholarship is convincing and her judgment balanced. There are virtually no typographical errors, and the editing is excellent; I found only two errors: Rasputin is called a monk several times (will that mistake ever cease to be made?), and Alexander I's religious commitments after 1812 are discussed without any mention of pietism, which was basic to them.

Theosophy came out of a history of occultism that is at least as old as the witch of Endor; its immediate ancestor was the Spiritualist movement that began in 1848 (again in New York) and was brought to Russia in 1852. The writer A. K. Tolstoi and the philosopher Vladimir Soloviev, among other notables, were interested in it; the chemist Dmitrii Mendeleev chaired a commission to study it and jumped the gun by publishing criticisms. When Theosophy appeared, a host of prominent Russians in the realm of thought, literature, painting, and music flirted with it or became followers of it or anthroposophy for years, sometimes for life. Some believers include Vladimir Soloviev and Vsevolod Soloviev, Nikolai Berdiaev, Andrei Bely, Konstantin Balmont, Nikolai Roerich, Wassily Kandinsky, Aleksandr Skriabin, Petr Uspenskii, Viacheslav Ivanov, Dmitrii Merezhkovskii, and his wife Zinaida Gippius.

One striking aspect of the movement was the role played by numerous women. Blavatsky was touched by scandal and fraud, including accounts of travels that she plainly never took and citations of works in languages she did not know. Quite different were, for example, Kamenskaia and Elena Pisareva, who were indefatigable and honest organizers, translators, and propagators of their faith, or Anna Filosofova, a prominent philanthropist and feminist who worked with peasants establishing schools and hospitals for the poor. Theosophy had a distinct orientation to social action; during World War I members of the society organized (vegetarian) soup kitchens, worked in hospital wards, and so forth.

Carl Jung called Theosophy, "together with its continental sister, Anthroposophy," "pure Gnosticism in Hindu dress." Both teachings tried in various ways to combine Indic and Western teachings. Their appeal rested on the claim to reconcile science and religion, knowledge and faith, and combination of the notion of fate (*karma*) with free will. Carlson has diligently worked in elusive and contradictory mate-

rials to illuminate a sadly neglected aspect of Russian cultural life in the last decades of empire.

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RICHARD STITES. *Russian Popular Culture: Entertainment and Society since 1900*. (Cambridge Soviet Paperbacks, number 7.) New York: Cambridge University Press. 1992. Pp. xvii, 269. Cloth \$44.95, paper \$14.95.

With this book, Richard Stites again demonstrates that he is one of the most creative and original historians currently writing in the field of twentieth-century Russian history. The book is a fitting sequel to his seminal study, *Revolutionary Dreams: Utopian Vision and Experimental Life in the Russian Revolution* (1989). Whereas *Revolutionary Dreams* focused on the hopes and visions of revolutionary activists and intelligentsia in the early years of Bolshevik rule, this study moves from the elites to "the masses." Focusing on urban songs and dances, light reading, live entertainment of all kinds, cinema, radio, television, musical genres with wide popular appeal, and even postcards, Stites seeks to rescue for posterity the cultural forms and practices enjoyed by ordinary people from 1900 to the early 1990s.

Stites traces, in broad strokes, the evolution of popular tastes as well as the preferences of Bolshevik cultural guardians who "took upon themselves the onerous and arrogant task of regulating the people's taste in a serious way" (p. 36). The tension between these two sets of cultural preferences and practices—those officially sanctioned and those arising spontaneously from the consumers of culture—provides one of the major themes for the book.

Stites's argument can be summarized briefly. After the Bolsheviks took power in 1917, they attempted to infuse popular culture with revolutionary content and to eliminate forms (such as *lubok* literature, saints' lives, knightly and religious tales, serial adventures, and bandit stories) of which they disapproved. He writes, "The communists did by fiat what the old intelligentsia had wanted to do: give the people what they thought was good for them and not what they wanted" (p. 41). During the Stalinist cultural revolution from 1928 to 1932, popular culture was nearly destroyed by self-righteous (intelligentsia) guardians of proletarian sensibilities.

But try as they might, the authorities could not legislate everything, and the desire for light entertainment persisted. Beginning in 1933–34, a major reversal took place and there was "an outpouring of spontaneous fun and joy for three or four years. Popular culture reemerged with great vigor" (p. 65). The leadership adjusted cultural policies to "capture mass audiences" and to respond better to the "new taste culture" of the recently arisen Stalinist elite. Then, in 1936, the authorities again began to sup-

press certain forms of entertainment, such as the *estrada* ("light live entertainment ranging from rural and traditional outdoor shows to modern nightclub revues"), whose levity accorded poorly with "solemn joy in the midst of heroic construction" (p. 80). And so it went, with alternating periods of control and relaxation, until Mikhail Gorbachev came to power and removed most official controls in the sphere of culture, popular or otherwise.

Before 1985, the cultural tastes of ordinary people were sometimes thwarted for considerable periods, and major forms of popular entertainment were curtailed or banned altogether. Nevertheless, according to Stites, popular preferences found expression even at the height of Stalinism and then reasserted their dominance in the era of glasnost. In Stites's telling of it, the story has a Bakhtinian, romantic emplotment: Communist mass culture oppressed genuine popular culture, but even in the bleakest moments, popular tastes managed to retain their vitality; finally, in the era of glasnost, the cultural preferences of ordinary people emerged victorious.

Stites's explanation for this phenomenon draws attention to two factors: the regime's need for effective channels of communication with "the masses" and the capacity of audiences to deploy their strategies as consumers of culture. The latter point brings to mind Michel de Certeau's book *The Practice of Everyday Life* (1984), which emphasized the ingenuity of ordinary people in coping with cultural forms imposed from above. Along these lines, Stites argues that even in the bleakest years of Stalinism, audiences that had no voice whatever in cultural policy could still influence the character and variety of some forms of popular culture (for example, films and books) by means of selective consumption.

This is a study in continuities. Stites develops a persuasive argument that certain cultural patterns, originating in the tsarist era, remained central to the popular arts throughout the Soviet period. These deeply rooted attitudes functioned like a deep structure of thinking among Russians and formed part of a "cultural code" between artists and entertainers on the one hand, and their audiences on the other.

Stites made a similar point in his earlier study, *Revolutionary Dreams*. There, too, he identified certain ingrained, traditional ways of thinking that originated in the prerevolutionary period and continued under Communism (for example, dualist thinking, nihilism, leveling, anti-intellectualism, and idealization of the peasant). In the present book, the focus is on self-attributed traits of the Russian "national character" that found their way into popular culture, such as openness or simplicity, combined with cunning and calculations; spontaneity and impulsiveness; moralism; the collective impulse; and social bonding. These self-characterizations appear time and again in the popular arts after 1917.

In the concluding pages, Stites writes that Russian popular culture "has reflected over time the feelings

and aspirations of people, their attitudes to the regime, and their vision of self" (p. 208). Considering the long decades of officially contrived and controlled cultural life under the Communists, this formulation raises many complex and controversial issues. Cultural specialists will be encouraged—maybe even provoked—by this book to undertake further research and analysis. When they do so, they will benefit from Stites's forceful arguments and his fascinating evidence and documentation.

Although the book is relatively short, it is a big book—big in ideas and in the extraordinary richness of the material. Stites writes with authority, verve, and humor. His book is required reading for anyone curious about Russia's cultural life in the twentieth century.

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G. A. GERASIMENKO. *Pervyi akt narodovlastiia v Rossii: Obshchestvennye ispolnitel'nye komitety (1917 g.)* [The First Act of Popular Power in Russia: The Social Executive Committees in 1917]. Moscow: Nika. 1992. Pp. 349.

This is an important monograph on the subject that delves into the fundamental nature of the Russian Revolution of 1917. Immediately after the February revolution, "public executive committees" sprang up and assumed de facto power in various provincial capitals, cities, *uezdy*, *volosti*, and villages. The importance of this subject has been recognized by such historians as William Rosenberg, Don Raleigh, and V. I. Startsev, but no comprehensive study has existed until the publication of this book.

Basing his argument on extensive use of hitherto untapped materials drawn from local publications and archival materials, G. A. Gerasimenko argues that the public executive committees constituted the most important locus of power in many localities throughout Russia in March and April 1917. Examining their social origins and political orientations, he defines public executive committees as "democratic power" that was more broadly based than *zemstvos* and city *dumas*, on the one hand, which were dominated by "privileged" classes, and the *soviets*, on the other hand, which excluded a large portion of the middle layers of society. According to Gerasimenko, public executive committees represented real "popular power [*narodovlastie*]," which attempted to "transcend class and party differences and carry out the policy that corresponded to the broader interests of all the democratic elements in society" (p. 133).

In his first two chapters Gerasimenko describes in detail the process of the emergence of public executive committees and of their expansion of power in March and April. These committees, formed spontaneously and without any directions from above, con-

tributed to the democratization of local administrative organs, but served as a major factor that caused the breakdown of the link between the center and localities. He further examines in his last two chapters the question of the public executive committees' relations with the Provisional Government and with the soviets. The Provisional Government refused to recognize public executive committees as official government organs. Rather, it challenged their authority by appointing its own commissars, which were instructed to establish an official local government network based on the zemstvos and city dumas. Public executive committees countered this move by reelecting their own commissars. As for their relations with soviets, both bodies initially cooperated and complemented each other. Soviets usually recognized public executive committees as a higher authority with jurisdiction over the matters that concerned the entire population. Nevertheless, after the Bolsheviks began to adopt the slogan "All power to the soviets" in April, tensions between soviets and public executive committees began to develop.

As the social polarization grew in the summer, the social basis of the public executive committees eroded. When the Provisional Government intensified its attack on the public executive committees by making their usurpation of governmental power a criminal offense, the soviets either withdrew their representatives from the public executive committees or intensified the pressure to control their activities. After the July Days the public executive committees were seriously weakened, and by the October revolution they ceased to exist.

Gerasimenko's findings challenge the traditional interpretation of dual power. He argues that the problem of power in the Russian Revolution should not be viewed merely from the conflict between the Provisional Government and the Petrograd Soviet, as has been done traditionally, but rather that it should be examined in the context of the more complicated and dynamic interplay among these two centers of power and the public executive committees. This view deserves serious attention and serves as a corrective to the general tendency of Western historiography on the Russian Revolution in the past two decades, which has been mostly preoccupied with the social chasm between the "bottom" and the "top."

Despite the unquestionable contributions that Gerasimenko makes in the interpretation of the Russian Revolution, I have two serious disagreements with his view. First, Gerasimenko treats public executive committees too mechanically. I suspect that the differences between urban committees and rural committees are so substantial that it is necessary to separate them. In the countryside, what triumphed was the strength of the village communes, whereas in the cities the demise of the public executive committees revealed the inherent weakness of the middle layers of society. Moreover, Gerasimenko ignores the nationality question that must have colored the com-

positions and policies of the public executive committees in the peripheries.

Second, I question Gerasimenko's positive characterization of the public executive committees as the embodiment of "popular power." In my view, they not only obstructed the Provisional Government from extending its authority to localities but also, by so doing, rejected the existence of the state's power itself. Having established a conclave of self-autonomous power in localities, they did not offer any positive alternative for the formation of the new state. Consequently, together with soviets, they contributed to the annihilation of any state authority and prepared the way for the resurgence of the Bolshevik dictatorship. Therefore, in my view, the public executive committees represented an inherent weakness of the Russian Revolution.

It must be noted that Gerasimenko's work is important also as an example of an emerging trend of new Russian historiography in the post-Soviet period. True, one can encounter some remnants of Soviet historiography in his terminology and methodology, but in its problem-setting and conclusions, it is a significant departure from the traditional Soviet approach. One should not be misled by the shabby appearance of this book; what is contained in this miserably manufactured book may be the harbinger of future creative work by Russian historians finally freed from the ideological strait jacket. Historians in the West have a great deal to learn from this work and can expect fruitful collaborative work with Russian historians in the hunt for "the true October."

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LEWIS H. SIEGELBAUM. *Soviet State and Society between Revolutions, 1918-1929*. (Cambridge Soviet Paperbacks, number 8.) New York: Cambridge University Press. 1992. Pp. xiii, 284. Cloth \$54.95, paper \$16.95.

The initial decade of the Bolshevik revolutionary experiment is at present a particularly active focus of innovative research in the history of Soviet Russia. Lewis H. Siegelbaum has produced solid work on a variety of topics of Soviet history and is the author of a particularly distinguished study of Stakhanovism. It is therefore doubly perplexing that the present volume should fall so far short of achieving the historical synthesis that is its announced purpose. In producing a book that is "as much a work of historiography as of history" that "necessarily blots out a good deal of 'what happened'" (p. 4), Siegelbaum, his erudition notwithstanding, produces a volume too abstruse for the uninitiated and redundant for specialists.

Siegelbaum approaches state and society as fluid entities. Within these broad configurations, the boundaries of ideology, politics, economics, and culture are in no way fixed, thus making internal state

and social frameworks capable of perpetual contention and redefinition. The relations between the two can be contradictory, he argues, but they are not in all cases antagonistic. And in the Bolshevik experience of 1918 to 1928, the dictates of retreat and recovery informed the process of reconfiguring the full development of the revolutionary state and society.

Within this framework, Siegelbaum employs a largely chronological approach through the five chapters that constitute the body of the book. His chapter on the civil war years reviews the cumulative effect of the crises that led to the adoption of the New Economic Policy (NEP) in 1921, although with only elliptical allusion to the development of the crises themselves. A chapter on 1920–21, slim and oddly out of proportion with the rest of the book, argues that the rationale for NEP was an economic retreat accompanied by an exercise in political consolidation. Subsequent chapters explain how the onset of NEP was understood differently by various groups, including the subdivisions of the party; that transforming society—from marriage law to religion to labor productivity—unleashed conservatism as well as revolutionary experimentation; that the disorganization of the late 1920s deteriorated into Joseph Stalin's cultural revolution.

In large measure, it is the ambitious scope of the work that is its undoing. One can neither question Siegelbaum's impressive command of the existing historiography nor fault his attempt to make it comprehensible conceptually. Yet trying to make singular sense of so diverse a literature leads him to commonplaces such as that "political institutions are shaped by the social environment from which they spring" (p. 1) and that "there have been many stateless societies, but the reverse is not the case" (p. 3). The discussion itself ranges so frequently into summaries of historiographical differences, without providing an explanatory framework, that the book will be virtually unreadable by nonspecialists and unusable in the undergraduate classroom. And the large number of doctoral dissertations and monographs presently in progress on the cultural and social history of the 1920s make gratuitous Siegelbaum's call in his conclusion for a new research agenda. Finally, substandard editing and the presence of too many typographical errors mar the text. Nevertheless, this work will provide a handy reference for specialists in Soviet history.

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NEAR EAST

F. E. PETERS. *The Distant Shrine: The Islamic Centuries in Jerusalem*. (AMS Studies in Modern Society, Political and Social Issues, number 22.) New York: AMS. 1993. Pp. 275. \$39.50.

This study builds on F. E. Peters's previous work in establishing a typology for Jerusalem and Mecca as

holy cities, and in viewing Jerusalem through the eyes of various visitors over the centuries. Peters focuses here on the thirteen centuries of Islamic rule over Jerusalem. Again interspersing his narrative with eyewitness accounts, he quite felicitously succeeds in writing the history of the city and in explaining the interplay between the sacred and the secular in the Muslim period.

To set the stage, Peters traces the development of the city from the era of David and Solomon through that of Herod and Hadrian, drawing attention to the urban layout established in the Roman period and to the importance of the huge platform atop Mount Moriah on which Solomon's Temple had stood and which remained the focal point of the city. He then proceeds chronologically, stressing the importance of the Emperor Constantine (and his mother Helena) in the location of various sites associated with the life and death of Jesus and in the building activities that established a new, sacred history for Christians in Jerusalem. After the Arab conquest (which, without any particular explanation, Peters says occurred in A.D. 635), Muslim shrines appeared on the Temple Mount, or *Haram al-Sharif*, by then coming to be associated with the sanctuary mentioned in Sura 17 of the Qur'an describing Muhammad's famous night journey from the sanctuary at Mecca to "the distant shrine." There is much interesting material in this section about the large stone outcropping around which the Dome of the Rock was built, and about the importance of the rock in both Jewish and Muslim traditions.

Peters argues that it was the Crusades that had the eventual effect of making not just the Haram area, but Jerusalem as a city, important to the Muslims. He notes that Muslim historians and chroniclers were unable to conceptualize and analyze the events of that era and therefore failed to understand why the Crusaders had come. Other Muslims, however, did begin to appreciate that the appropriate response to the Crusades was not only military but also ideological and religious, and that a jihad to retrieve Muslim lands was necessary. Zengi, then his son, Nur al-Din, and especially Saladin began the process of undertaking a religious war against the Crusaders, and later of restoring Islam to Sunnism after a century of Shi'ite governments throughout the domains of Islam. The Ayyubids and especially the Mamluks after them made Jerusalem into an Islamic city and not, as Peters puts it, merely a city with a Christian majority that contained a major Muslim shrine. Madrasas, Sufi convents, and many other typically Muslim buildings and institutions proliferated, most of them made possible by waqf endowments. In the early Ottoman period, Sulayman the Magnificent rebuilt the city's walls and otherwise invested in Jerusalem. Peters stresses, however, that whatever dynasty controlled Jerusalem, the city had little political or economic significance, and that its holy places served primarily

as "a kind of *point d'appui* . . . to focus and justify not presence but *intervention*" (p. 238).

Readers who enjoyed Peters's *Jerusalem: The Holy City in the Eyes of Chroniclers, Visitors, Pilgrims and Prophets* (1985) will find much repetition in this study. Quotations from the primary sources are again lengthy, often running for a page or more, but here they are usually not precisely identified. Although this book seems to be written for a wider audience than some of Peters's other works, it still seems presumptuous to refer the reader to a previous study for information that could easily have been included. Moreover, the way the book is typeset makes it difficult to distinguish between the narrative and the quoted material. Careless editing sometimes results in confusion, for example regarding John of Wurzburg and al-Harawi, who are obviously twelfth and not eleventh-century figures. Maps enhance the book, although some of them contain topographical features that look like smudges; and it would have been useful to have included a reproduction of the Madaba map. There is an excellent appendix on the sources for the history of Jerusalem, and, in general, Peters continues to contribute to our appreciation and understanding of that holiest of holy cities.

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DAVID MENASHRI. *Education and the Making of Modern Iran*. Ithaca: Cornell University Press, with the cooperation of the Moshe Dayan Center for Middle Eastern and African Studies, Tel Aviv University. 1992. Pp. xvii, 352. \$47.50.

One of the neglected aspects of Iranian history under Pahlavi family rule (1925–79) was the introduction, role, and outcome of modern mass education and higher education. David Menashri's study fills this lacuna to a large extent.

The author analyzes "the contribution of education to the overall process of social, economic, and political change in the two centuries since the country was exposed to Western schooling," primarily focusing on higher education since the advent of the Pahlavis (pp. xi, 3). Menashri also seeks to examine modern education in Iran as a "catalyst and litmus test for change, as well as an element itself subject to change" (p. 3). He relies on primary sources and personal interviews with educators employed by the Pahlavi regime. His rich compilation and analysis of statistics relating to Iran's educational system throws much light on the paradoxical growth of this modern element, which ultimately undermined and served a death blow to its own architects. No doubt his analysis would have been richer and less handicapped had he not relied solely on official educators but also included opposition educators.

Moreover, the author's repeated inaccuracies in

transliterating and translating Persian words sheds some doubt on his command of the Persian sources. (See, for example, pp. 16, 67, 93 n. 13, 109, 112.) Other errors reinforce this impression and point to a rudimentary acquaintance with Iran's political history: the confusion between Taymur Bakhtiar, the first chief of the shah's intelligence organization, SAVAK, with his cousin Shapour Bakhtiar, the National Front oppositionist who became the monarch's last premier in 1979 (p. 157); Dr. Arani, the Marxist thinker and educator, was a physicist, not a physician (p. 141); the communist newspaper *Paykar* was not a pliant client of Soviet Russia (p. 141).

Briefly covering the pre-Pahlavi educational reforms (pp. 27–90), Menashri examines the "Westernization" of Iranian education under Reza Shah (1925–41) before moving on to study its rapid expansion under the second Pahlavi king (1941–79). For both these periods, the author accepts official, often propagandistic, pronouncements of the regime at face value as if they were sincere wishes or well-planned objectives of the Iranian rulers. He fails to see that, stripped of their bombastic rhetoric, the reform policies often reflected responses to social pressures and political dangers that threatened the very existence of the Pahlavis. Indeed, Menashri's own evidence demonstrates that the initiatives for "home-grown" higher education were closely related to communist agitation in Germany and France among young Iranians, like Arani, sent to Europe for university education. Similarly, the shah's decision in October 1967 to establish a new ministry for higher education and the plan to expand the university system (p. 211) were immediately linked to the growing opposition to the Pahlavi monarchy by Iranian students abroad.

The reversal of the same decision in the early 1970s also was politically motivated, insofar as home universities had become hotbeds of revolutionary activity against the shah (p. 211). Although aware of the "political" nature of these decisions (pp. 145, 211 n.3), the author fails to treat them in depth and to draw the necessary conclusions: that the modernization of Iran's educational system, whether accelerated or decelerated, was to be history's kiss of death for the Pahlavi monarchy.

As to the "achievements" of this modernization process, Menashri is lucid enough: "In quantitative terms, elementary and secondary education had indeed gradually turned into a success story, but taken as a whole, Iran's educational system failed to formulate an appropriate educational philosophy. For rapidly growing numbers, high school education became . . . a 'bridge to nowhere'" (p. 209).

In spite of his deep-seated aversion to Islam (pp. 21, 25, 99), Menashri recognizes that the failure of higher education in promoting research under the monarchy did not result from the "significant limitations" that "the traditions of Islam" imposed on it, but rather from "the social, economic, and political cir-

cumstances in which the new academic centers had sprung up" (p. 231). Furthermore, in spite of his prejudice in favor of modern education, he acknowledges that in the Islamic world "the concept of knowledge enjoyed an importance unparalleled in other civilizations" (p. 15), and "original writing (and its publication) [had been] important cornerstones of traditional education" (p. 230).

Menashri also recognizes another limitation of modern education under the Pahlavis: the absence of academic freedom. Except for brief periods, the Iranian universities were under state control and, consequently, "Fear, frustration, and lack of security prevented the development of creative intellectual activity at the institutes of higher learning and significantly restricted their contribution to a possible Iranian cultural revival in a modern mold" (p. 298).

Yet the author concludes that modern education made "some important contributions," namely, that "the religious establishment lost its age-old monopoly over education at all levels," that "the [clerical] revolutionary leaders [in power] neither tried nor even expressed the wish to wholly reverse the process," and that, in spite of its "shortcomings, [modern] education has undoubtedly been one of the most important—if not the most important—social instrument shaping modern Iran" (pp. 301, 303). In light of the results observed, and admitted by the author, the question to ask is whether the founding Pahlavi succeeded in his modernization dream: "I don't want to turn the Persian into a bad copy of a European."

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AFRICA

ELIZABETH SCHMIDT. *Peasants, Traders, and Wives: Shona Women in the History of Zimbabwe, 1870–1939*. (Social History of Africa.) Portsmouth, N.H.: Heinemann or Baobab, Harare, or James Currey, London. 1992. Pp. xiii, 289. \$22.95.

In this contribution to the burgeoning historiography of Zimbabwe, Elizabeth Schmidt locates herself in Goromonzi District, not far from the capital, Salisbury/Harare. In the first part of the volume, she surveys the precolonial society and economy, the local experience of the Chimurenga uprisings of 1896–97, and the effects of colonial demands on the household economy of rural Shona people. She stresses women's initiatives as peasant innovators, together with their capacity within the confines of colonial reserves to cope with the increasing absence of their men.

Almost exactly in the middle of the text, the focus shifts. Following a summary of "loss of status under colonial rule," Schmidt turns to the means available to women to escape the drudgery and intensified exploitation of the "customary" realm. She follows runaway women "to missions, mines, farms and towns." She

discusses the stereotypes of the European colonial outlook, so amply reflected in missionary and official documents. African women had to be domesticated and absorbed into a safe, colonially dominated order as Christian wives or domestic servants. Colonial courts became linked in the effort to regulate marriage-making. The volume ends with a lively discussion of the contradictions of colonial life, wherein the sexual threat of black men to white women loomed large in what has appropriately been described as episodes of colonial moral panic. White women, however, preferred male servants in part for reasons of sexual jealousy. Schmidt exposes and deplores the racism of criminalizing black men for alleged violations of white women while white men enjoyed virtually total license to use black women.

Eastern and Southern Africanists, and probably historians of the U.S. South as well, will find much that is familiar. Schmidt's originality is greatest where she concentrates on Goromonzi and draws on the documentation and influence of the Jesuit Mission centered at Chishawasha and the Methodist Mission at Epworth. Within these arenas the written record and memories can be brought together. Her oral data include some seventy interviews she conducted, supplemented by much material collected by others in the 1970s. One would have welcomed a more critical discussion of the oral historical sources, given that they figure so much in the content as well as the claims for the work. The promise to retrieve women's voices remains largely unfulfilled. Women have been consulted, but the reader still finds only fragmentary evidence of the lives and attitudes of these women either as actors or informants. The remarkable collection of women's accounts contained in *Mothers of the Revolution*, edited by Irene Staunton (1990), makes for a dramatic contrast, although its introduction also is short on methodological reflection.

Hard on Schmidt's heels is Diana Jeater who, in *Marriage, Perversion and Power: The Construction of Moral Discourse in Southern Rhodesia, 1894–1930* (1993) also works from a district base (colonial Gweru) but brings greater conceptual clarity to the history of gender constructions and sexuality as a colonial obsession. The comparison underscores the conclusion that, had Schmidt not wandered away from her Goromonzi case, her contribution to the history of women's evolution from peasants to colonized wives might have been more telling. Nevertheless, the availability of this book as a paperback puts it within reach of students. It has merits as a text for the study of gender and racialism in colonial Africa. There are nice summaries of the literature, debates, and research in progress as they stood in the 1980s. The copious bibliography is especially to be recommended.

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ASIA

SAMUEL HUGH MOFFETT. *A History of Christianity in Asia. Volume 1, Beginnings to 1500*. New York: HarperCollins. 1992. Pp. xxvi, 560. \$45.00.

The history of Christianity in the lands east of Palestine is a fascinating and complex story that is not well known. Both because of the difficulty of the sources and also because Christianity in these areas faded into insignificance, few have attempted the daunting task of writing a comprehensive history. The last such endeavor was Aziz Atiya's *A History of Eastern Christianity* (1968).

Samuel Hugh Moffett's first volume of a projected multivolume work covers developments up to 1500. These are discussed in three parts: from the Apostles to Muhammad, from Alopen to the Crusades, and from Chinggis Khan to Tamerlane. In most respects this is an excellent text with a highly readable narrative. The author's coverage is both comprehensive and balanced, and his scholarship is impressive. Highlights are apposite quotations from both primary sources and secondary discussions. Five maps, indexes, and an appendix of the text of the famous Nestorian monument inscribed in China in 781 are also included.

Christianity in the East survived under the shadow of the centuries-long conflict between the Roman West and the Persians, who embraced first Zoroastrianism and then Islam. The Church of the East, better known as the Nestorians, succeeded in establishing outposts in India and in China. Moffett admirably clarifies the personal and theological differences among the Byzantine (Melchite), Jacobite (Monophysite), and Nestorian Christians.

In his conclusions (pp. 503–09), Moffett offers some helpful insights into the plight of Christianity in Asia. He suggests that a major cause of difficulties was the failure to translate the Scriptures into Arabic or Chinese. Another factor was the contempt that one Christian sect had for all the others. Despite the fact that the Persian Nestorians who brought Christianity into Asia were themselves Asians, they were nonetheless always considered by the Chinese to be "foreign" Asians.

Moffett, who was born in Korea and has taught in China, demonstrates a marked empathy for an Asia-centric view in accepting the traditions, which ascribe the early establishment of the church in Syria to Addai (pp. 50–51), and the church in India to Thomas (pp. 35–36, 39). In these issues he wants to err on the side of qualified acceptance rather than skepticism. In support of the Indian tradition, Moffett cites (p. 40, n. 4) the fact that, "The Thomas tradition is accepted by almost all Indian Christian writers and by an increasing number of secular historians as well." He fails to consider that some of the secular historians whom he quotes may be influenced by nationalistic biases.

Some readers may disagree about the depth of

coverage on subjects that may be dear to their hearts. Armenia, a buffer state, which became one of the first kingdoms to embrace Christianity, is given rather short shrift as an area that eventually came under Byzantine orbit (pp. 8–9). But certainly in the late Roman empire Armenia was very much under Persian (Parthian) influence. The author does not do justice to Christianity among the Arabs before Muhammad, nor does he refer to Irfan Shahid's important monographs on the subject.

Moffett's treatment of the rival Manichaean movement is rather unsatisfactory. He does not mention the Cologne Codex of Mani's life or the researches of Werner Sundermann on the important Turfan documents. Although he cites a 1979 work by Samuel N. C. Lieu, he fails to note the latter's important study, *Manichaeism in the Later Roman Empire and Medieval China* (1985). He does not consider the possibility that among the groups persecuted by Kartir (p. 109) were the Mandaeans. In his brief references to the Maronites, the most important Christian community in Lebanon, Moffett neglects to mention their service to the Crusaders.

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BRIAN E. MCKNIGHT. *Law and Order in Sung China*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1992. Pp. xiv, 557. \$74.95.

In his detailed exploration of law enforcement and penal policy during China's Sung dynasty (960–1279), Brian E. McKnight pursues two desirable yet redoubtable goals, either one of which might well have warranted a volume in itself. The first goal is to describe how these related institutional phenomena changed (or did not change) over the course of the Sung. The second (and markedly more demanding) goal is "to reveal their ideological foundations and the interactions of these ideological factors with political, social, and economic conditions in determining the shape and functioning of these systems" (p. xii). McKnight's study, to a remarkably satisfying degree, impressively achieves both of these daunting objectives.

McKnight argues that law enforcement in traditional China is best understood as having been a dimension of social control. He brings forward a compelling array of preliminary analyses in support of this assertion, ranging from longstanding Chinese philosophical assumptions about order to the pivotal importance of the emperor as the exemplar of order to the role of law as order's last line of defense. McKnight tacitly indicates that whereas certain traditional ideas about law enforcement have predictably Legalist underpinnings (that is, suppression), many others (persuasion, prevention, and cooptation, for

example) represent Confucian (or perhaps even "proto-Confucian") responses to deviance. He postulates that social disorder during the Sung was most likely to be met with a consistent succession of responses, with suppression used only as the last resort. Of equal importance to McKnight's "law enforcement as social control" paradigm is his introduction of the actor that he deems so crucial to the system's success: the intendant, a supervisory law-enforcement official who was the intermediate effector of order. Indeed, McKnight's deliberations on the recruitment, activities, and transformation of the Sung's large cadre of such middle and lower-level functionaries as the intendant serve as the centerpiece of his book.

In an illuminating discussion of major types of crimes and criminals, McKnight informs us that, because of the empire's geographical as well as its (often underappreciated) ethnic and religious diversity, to "speak of 'crime in Sung China' is somewhat misleading" (p. 105). He also notes, however, that the ruling elite—the creators of the legal code—maintained a uniform punitive outlook toward society's criminal element. They were most alarmed by those types of crimes and criminals that most threatened their own positions and values; they were most troubled by acts committed by their own members that undermined their position (corruption) and by acts of outsiders that threatened to weaken the hold of the rulers on the ruled (banditry). But McKnight is also quick to expose the fallacy of overestimating the extent of local control exercised by Sung central authority. Despite the fear with which the elite regarded such potentially seditious manifestations of criminality as corruption and banditry, such crimes were often obviated or even prosecuted through informal or semiformal channels, such as the *pao-chia* system of mutual security.

Having made a compelling case for the effectiveness of the Sung law-enforcement system in his earlier chapters, McKnight logically turns, in his later chapters, to the other buttress of Sung law and order, its penal system. McKnight finds that the key impetus behind the Sung penal system was the universally accepted idea of deterrence. Consequently, although the rehabilitation of wrongdoers was not an altogether neglected aim of punishment, the system nonetheless tended to prescribe penalties that were so ghastly that they in effect discouraged wrongdoing by incapacitation. The Sung penal system, probably more completely than its law-enforcement system, drew much from the ancient past's practices of bodily humiliation, if not disfigurement; yet, as McKnight shows, in keeping with the strong civilizing attitudes of the time, public mutilations were increasingly replaced by public beatings.

Throughout his chapters on the penal system (as elsewhere), McKnight only implicitly notes many points of rather extreme cultural divergence between China and the West. For example, in his discussion of jails and jailers, he remarks—with no hint of irony—

that imprisonment in the Sung, which was used primarily as a form of incarceration prior to either trial or punishment, "was not thought of as a punishment in itself" (p. 353). McKnight also never establishes how or why some of the features of Sung culture (for example, the arts, theater, and printing) that are discussed as subtopics in the chapter "The Historical Context" bear directly on his expressed theme of law and order. Nevertheless, these small flaws scarcely detract from McKnight's inestimable achievement; his book will serve as a fitting template for future law-and-order studies of other Chinese dynasties for generations to come.

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JUNG-FANG TSAI. *Hong Kong in Chinese History: Community and Social Unrest in the British Colony, 1842–1913*. New York: Columbia University Press. 1993. Pp. xiii, 375. \$45.00.

Jung-fang Tsai has written a carefully researched book that places the Hong Kong Chinese into the broader context of Chinese history between the end of the First Opium War (1842) and the failure of the first Chinese Republic (1913). Tsai feels that most Hong Kong histories have taken a colonialist point of view, so he concentrates on the Chinese elite and working classes. He finds evidence everywhere in the Hong Kong Chinese community of the familiar Neo-Confucian pattern of strong vertical subordination. This pattern characterizes the kinship, community, and dialect groups that became the basic building blocks in urban social organization. Tsai shows how broad coalitions emerged first among Chinese merchants and then through short-term alliances in the working classes. He argues that the merchant leadership cooperated best when furthering their collective business interests; working people also considered their economic interests first, but, like the elite, increasingly moved to resist British colonialist rule.

Tsai's characterizations of Hong Kong follow closely the work of recent studies in Chinese urban history, reinforcing his main argument that Hong Kong was a part of China, not a separate colonialist enclave. Following a suggestion by Joseph Esherick and Mary Rankin in *Chinese Local Elites and Patterns of Dominance* (1990), Tsai secondarily tries to show that Hong Kong had special characteristics. What was different in Hong Kong, according to Tsai, was the power of the modern colonialist state and the heightened sense of resistance that all classes of Chinese felt to it. On this point his interpretation warrants closer attention.

Tsai writes that British colonial rulers preferred indirect rule in Hong Kong, depending on Chinese elite collaboration until riots connected with the 1884 Sino-French War caused the British to assume more direct rule. Thus, the power of the Chinese elite was

severely constrained by the modern colonialist state. Tsai seeks to explain the continuing strong collaborationist positions taken by the Chinese elite after 1884. He presents the usual arguments that practical economic concerns and the acceptance of Neo-Confucian principles of behavior led Chinese elites to collaborate, but he nevertheless sees the Hong Kong situation shaped by an unusually strong colonial state. The overwhelming evidence from treaty ports, other colonial enclaves, and the wide variety of urban situations throughout China suggests to me, however, that from the 1840s on Chinese urban leadership had displayed a strong collaborationist character whatever state—weak or strong, Chinese or foreign, modern or traditional—it has faced.

Tsai's third major concern is nationalism in Hong Kong. He concludes that the Chinese in Hong Kong, because of their colonial situation, developed earlier, stronger, and broader forms of nationalism, both among the elite and the working classes, than elsewhere in China. Tsai did extensive archival research to build up his account of resistance to British colonial rule. He concludes that Hong Kong workers never recklessly resisted colonialism; still, he finds nationalism did take root in all classes of Chinese. Especially after 1895 coolie nationalism becomes evident, although it was still tied to practical concerns or regional self-assertion. Here Tsai's decision to end his coverage at 1913 means that he did not investigate the considerable Hong Kong working-class strikes and activism that emerged after 1919, when worker actions gave the impression of being in the vanguard of Chinese anti-imperialism. Yet until 1913, the history of Hong Kong working-class activities, like those of the merchant elite, seem comparable to Chinese workers elsewhere.

This observation, however, only reinforces Tsai's primary conclusion that Hong Kong's history cannot be separated from broader trends in China. Thus, the book makes a powerful argument against the concept of treaty-port exclusiveness, most forcefully argued by Rhoads Murphey ("Treaty Ports and Chinese Modernization," in Mark Elvin and G. William Skinner, eds., *The Chinese City between Two Worlds* [1974]). He shows that, even in the most colonial of situations after 1842, the elite and the general population in Hong Kong shared the same patterns and characteristics found elsewhere in urban China.

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ELIZABETH J. PERRY. *Shanghai on Strike: The Politics of Chinese Labor*. Stanford: Stanford University Press. 1993. Pp. ix, 327. \$35.00.

"Different workers engage in different politics," argues Elizabeth J. Perry (p. 239) in this superbly researched and tightly conceptualized study of Shang-

hai labor militance. She shows how complicated, contingent, and just plain chancy the operations of this deceptively simple maxim can be. Covering the 110 years before the successful communist revolution in 1949, Perry's study gently modifies several generations of received wisdom about Chinese labor activism. Jean Chesneaux and most Chinese historians of the labor movement have seen worker radicalism as fueled by classical Marxist processes of proletarianization and industrialization and ignited by the intervention of communist organizers. A more recent generation of Western scholars has begun to catalogue worker fragmentation, which was aggravated by differences in native-place origin, gender, skill level, and workplace size, as well as by political repression and economic pressures. Perry discards none of this earlier scholarship, but she challenges many of its conclusions. She traces the making of an industrial proletariat, but she also notes that many of Shanghai's most militant workers were skilled laborers with the habits and organizational strengths of artisans; they were often the most receptive to communist appeals. Similarly, she acknowledges fragmentation among workers but demonstrates conclusively that a multiply divided work force with no class consciousness could still make plenty of labor trouble.

This book is divided into three parts. "The Politics of Place" recapitulates and extends what we know about Shanghai's immigrant work force. Artisans from South China were recruited into the city's earliest mechanized factories, bringing with them a strong guild tradition reinforced by native-place ties. These workers were militant both in defense of their own working conditions and in support of larger political causes. The much poorer "North China proletarians" (North China meant any district north of the Yangzi River) entered the work force as unskilled "lumpenproletarians" or factory laborers. Unskilled but not unorganized, most swore allegiance to a boss in the Green or Red Gang, and these gang affiliations, like the guilds, could become the basis for protest.

Part 2, "The Politics of Partisanship," chronicles interconnections between workers and political parties from the May Fourth Movement of 1919 to 1949. Communist successes in 1920s union organizing have been chronicled elsewhere, but Perry shows just how crucial gangster connections were in extending communist influences among unskilled and semiskilled factory workers. Artisans remained the most reliable source of support for the communists. When gangsters shifted their allegiance to the Guomindang during the Nanjing Decade (1927–37), they set themselves up as power brokers who could deliver—or withhold—labor support. Men like Du Yuesheng controlled government-approved "yellow unions," which were strongest among semiskilled workers and often far from docile. During and after World War II, communists expanded their contacts among skilled

workers and rebuilt gangster connections, to good political effect.

Part 3, "The Politics of Production," examines the effect of the production process itself on labor protest in three industries: tobacco, textiles, and transport. Within each of these industries, Perry finds that worker militance varied by skill level, with the most skilled workers generally the most active. Gender and native-place divisions were also important, but they were reinforced by factory organization that defined "skill" partly as a characteristic of men from particular districts. Temporary alliances sometimes united skilled, semiskilled, and unskilled workers; nevertheless, pursuit of distinct interests rather than class unity was the norm.

This brief summary cannot do justice to the subtleties of Perry's argument, to the thoroughness of her research (which includes much hitherto unavailable archival material), or to the deft comparisons she makes with European and American labor history. She combines the social scientist's passion for order with the historian's eye for compelling anecdote. Thus, we learn not only about large-scale intersections of native-place identification, national politics, and the production process but also about the communist organizer who slipped on a watermelon rind as he tried to flee the police, and the "frowning movement" conducted by disaffected department store employees. The result is a sophisticated, provocative, and entertaining piece of work that profoundly alters our understanding of Chinese labor.

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NAOKI SAKAI. *Voices of the Past: The Status of Language in Eighteenth-Century Japanese Discourse*. Ithaca: Cornell University Press. 1992. Pp. xiv, 349. \$35.00.

Naoki Sakai's study is one of the most daring and ambitious books ever written in English about Japan. It is ostensibly a study of a dramatic and broad-gauged change that occurred in eighteenth-century Japanese philosophy, literature, and drama, a change in what Sakai calls the "mode of discursive formation" or, more simply, the prevailing "conception of language." But this book is also an exploration of the larger theoretical questions raised by studies such as this: what is a "language"? What is an "I"? And what do we mean when we say that someone "belongs to a language"? Sakai's willingness to problematize the very concepts he uses in his analysis of historical material makes this an unusually complex and daunting book.

Sakai discerns the first evidence of the sea change in the writings of the Kyoto Confucian Itō Jinsai (1627–1704). He sees Jinsai challenging existing conceptions of language and offering a new conception in its place. He tells us that although Jinsai's major

target was Neo-Confucianism, he continued to use a Neo-Confucian vocabulary but did so parodically, which allowed him to produce new notions of self and social relations. More concretely, Jinsai argued for the importance of the body as the medium of practice, as the site where virtue was realized, and as the basis for reciprocal relations with one's community. Sakai also casts Jinsai's formulations in another way. Using the narrative theorist Emile Benveniste's terms, he characterizes the Neo-Confucians as reducing things to the "enunciated" and Jinsai to "enunciation." And so, according to Sakai, it is Jinsai's conceptions of the centrality of the body, practice, community, and "enunciation" that comprise the key elements of his new language and signal the change that Sakai believes took place in the late seventeenth century.

Contemporary literature is a second site where Sakai discerns this change. He points to a new "parodist literature" represented by new dramatic and poetic forms, chiefly puppet plays and comic *haikai*, that favored a narrative mode Sakai calls the *katari* mode and affirmed speaking over reading and pointed to the inadequacy of written texts. The new literature offered an antidote to the deficits of texts: performance or, even more fundamentally, the body. Here Sakai detects a new kind of intertextuality, one that found authors moving between representational (reading) and situational (speaking) spaces and attempting to return to "enunciation," that imagined state when speakers were not simply the subjects of recorded utterances but were still united with what they said.

Turning from literature to philosophy, Sakai also situates the Confucian scholar Ogyū Sorai (1666–1728) and the nativists Kamo Mabuchi (1697–1769), Motoori Norinaga (1730–1801), and others in the middle of this change. They too favored speech, the body, performance, and "enunciation" and tried to overcome the "temporal distance" implicit in the relationship of speech and writing. Their main concern, however, was overcoming the distance that separated "past" and "present" and recovering the past. Clearly, the "past" here was "the past as speech, enunciation, and performance," not the past as "writing, enunciated, and representation." As Sakai points out, this was a complicated move, for two reasons: first, because "enunciation" in this instance was not the "enunciation" that is normally thought to precede the "enunciated" but was actually, as he puts it, the "product of the enunciated," the product of the very split between "enunciation" and the "enunciated"; and second, the goal of recovering the original "enunciation" was a chimera, since the most one could hope to recover was a "representation" of the past, or a text. But Sakai believes that this valiant attempt to recover a lost and irretrievable past was important as an affirmation of life (speech, phoneticism, and enunciation) and a denial of death (writing, ideography, enunciated).

Both the strengths and the weaknesses of Sakai's book originate in its presentist orientation. To the early modern material Sakai has brought an impressive knowledge of contemporary notions of textuality; an understanding of current theories of writing, reading, and speaking; and a healthy respect for deconstruction. The result is a truly novel and interesting reading of material well known to specialists in the field. Yet Sakai's presentist assumptions will trouble some readers. The problem is that much of what he says simply does not ring true. His discussion of the Confucian material, for example, sounds distinctly un-Confucian and seems misleading. And those familiar with the recent scholarship on Tokugawa intellectual history will be troubled by Sakai's reversion to an old and generally discredited interpretive position, one that has Jinsai doing battle directly with the twelfth-century Chinese philosopher Zhu Xi (1130–1200), and by the implicit rejection of the view that Jinsai was responding to newer varieties of Neo-Confucianism that developed in China and Korea long after Zhu Xi's death.

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TAKIE SUGIYAMA LEBRA. *Above the Clouds: Status Culture of the Modern Japanese Nobility*. Berkeley and Los Angeles: University of California Press. 1993. Pp. xvii, 430. \$45.00.

Takie Sugiyama Lebra has written the seminal work on the social characteristics of Japan's modern aristocracy. Officially formed in 1884 to include 509 families, that body had nearly doubled in size by 1947, when it was forced to dissolve under the aegis of the postwar constitution. Lebra relies primarily on 101 interviews (with survivors, descendants, and collaterals), on written documentation (retrospective official records, autobiographies, and biographies), and on some participant observation to reconstruct in rich and complex detail many aspects of aristocratic society. She is principally concerned with issues central to her discipline of anthropology and organizes the book around these topics: the aristocracy's relations with its ancestors, the ongoing struggle to resolve succession crises, the social and material characteristics of aristocratic lives, marriage patterns, and the acquisition of aristocratic manners through socialization practices inside and outside the home.

Two features of Lebra's study will be of special interest to historians: her sketch of the origins of the modern aristocracy and her implicit portrait of its historical destiny. When Japan's modern state was formed in 1868, the country already had a hereditary court aristocracy that had emerged over 1,200 years earlier. After thriving during the Heian Period (794–1185), that aristocracy had fallen on hard times and was a politically powerless, socially effete, economically penurious body by the late nineteenth century.

Nonetheless, devotion to an imperial government obliged leaders of the new state to preserve this body. In keeping with the reformist spirit of that era, however, they combined it with two other social groups: the former regional barons (*daimyō*) who had governed provincial Japan during the Tokugawa era (1600–1868), and men of merit who had contributed to the achievements of the Meiji state (government officials, military officers, academicians, and businessmen). This amalgam of proud courtiers, dispossessed warriors, and ambitious parvenus constituted the aristocracy that operated just below the apex of Japanese society—the imperial institution—for sixty-three years.

It is not surprising that Lebra implicitly portrays an aristocracy internally divided and wary of its fellow members. Hereditary court aristocrats were clearly the most standoffish; many of them have preferred lives of strict decorum and genteel poverty. The former military lords were a disparate group that included many with aristocratic pretensions, if not manners, and others with a fiercely pragmatic approach to life's demands. The men of merit and their successors seemed to appreciate noble honors but in most cases were not crippled by them. Lebra offers many glimpses of the opulent and privileged lives that these families led before 1945. The reminiscences of her respondents accentuate how pronounced were the social, political, and economic inequalities of prewar Japan, and how far many of these families have fallen since the war. Stripped of nearly all their properties, forced to work in an aggressively capitalist economy, removed from the formal political arena, and marrying overwhelmingly outside their own ranks, the descendants of this aristocracy are in most cases—but not all—gradually blending into Japan's affluent, "middle-class" meritocracy.

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CARL BOYD. *Hitler's Japanese Confidant: General Ōshima Hiroshi and MAGIC Intelligence, 1941–1945*. Foreword by PETER PARET. (Modern War Studies.) Lawrence: University Press of Kansas. 1993. Pp. xviii, 271. \$25.00.

Among the studies of World War II communications intelligence that have appeared since the 1970s, Carl Boyd's book is a welcome contribution. He shows how, during World War II, Ōshima Hiroshi, the Japanese ambassador to Germany, provided the United States with one of its most important sources on the plans of Hitler and other high Nazi officials. At the end of 1940 the U.S. Army Signal Intelligence Service broke the Japanese diplomatic code, but Ōshima never discovered that the Americans had decoded the nearly 2,000 messages he transmitted to Tokyo. Until recently the messages, a small portion of the mass of German-Japanese Magic (the cover name

used by the U.S. for all intelligence produced by the solving of foreign codes and ciphers) remained classified.

Ōsihma had been in Germany since 1934, first as military attaché and then as ambassador. His government recalled him in 1939 but posted him to Berlin again the following year, where he remained until the final days of the war. Boyd emphasizes two themes and their connection: the close relationship and common interests Ōsihma had with Hitler and his foreign minister, Joachim von Ribbentrop; and the remarkable achievements of American cryptanalysts in breaking the Japanese code and deciphering Ōsihma's messages.

The messages filled a crucial gap in Allied intelligence in Europe left by Ultra, the intelligence Britain obtained from breaking the German code enciphered on the Enigma machine. The British were unable to read the secret communications of the top Nazi leaders, something provided by the Magic intercepts of Ōsihma's messages because Hitler and von Ribbentrop confided significantly in the ambassador. As a result, Boyd concludes, "the labor of American cryptanalysts turned Ōsihma into an inadvertent informer of incalculable importance in leading the Allies to victory" (p. 16).

Boyd makes a powerful case that the information in Ōsihma's communications served Washington more than Tokyo. The ambassador's lack of objectivity in seeing the war too much from German and Japanese perspectives, in overestimating German strength, and in failing to think on a world-wide scale limited his usefulness to his own government. As for Ōsihma's value to the enemy, during 1941 Magic intelligence obtained from him information about Nazi plans to attack the Soviet Union, the progress of the war there, and what turned out to be Japan's attack on Pearl Harbor.

During 1942 the Ōsihma intercepts yielded important information to Washington on conditions in Nazi-occupied Russia, the planned German buildup in North Africa, Japan's decision not to attack the Soviet Union, and the elaborate Axis strategy to advance on the Middle East. At the end of the year his messages to Tokyo showed that he believed the tide had turned against the Germans on the eastern front. In 1943 the Americans learned from Ōsihma valuable material on the rumored German-Soviet separate peace and in 1944 on German fortifications in northern France.

Finally, Boyd demonstrates how, despite elaborate preparations, President Franklin Roosevelt had only limited access to Magic intelligence when he was away from Washington, as he so often was after 1942. Until 1944, Boyd observes, the failure to distribute such information speedily and completely "was the Achilles heel of the system" (p. 101) and hurt its effectiveness in helping to end the war sooner.

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MARK P. PARILLO. *The Japanese Merchant Marine in World War II*. Annapolis, Md.: Naval Institute Press. 1993. Pp. xx, 308. \$28.95.

Naval history usually discusses sea battles and admirals, and studies of the war against Japan have been no exception to this rule. The standard accounts almost always devote most of their attention to the battles of the Coral Sea, Midway, Savo Island, Leyte Gulf, and the rest. Yet, as Mark P. Parillo shows, the Japanese defeat in the Pacific cannot be explained solely by reference to strategy, operations, and tactics.

At the end of 1944 the United States and Japan fought the largest naval battle in history at Leyte. Yet by that time the Japanese had effectively lost the war. Coal imports to Japan had declined from 2,300,000 tons at the end of 1941 to about 600,000 tons at the end of 1944; imports of pig iron and bauxite had declined by more than 85 percent and rubber imports were zero. Only 67 percent of the supplies and munitions dispatched by Japan to her far-flung garrisons in the Pacific and Asia ever reached their destination. The cause of all this was the destruction of Japan's merchant marine by American forces in a period of less than two years, between 1943 and late 1944. Having begun the war with one of the largest and most modern merchant fleets in the world, Japan suffered the near annihilation of all its shipping, including replacements built during the conflict, a catastrophe that insured its ultimate defeat.

This critical development, when not omitted entirely in favor of more dramatic sea fights and island battles, is usually explained by reference to the United States' outstandingly successful submarine campaign and Japan's appalling failure to devise effective measures for trade protection and antisubmarine warfare. Parillo does this aspect of the story full justice, drawing on little-known Japanese-language publications as well as U.S. intelligence reports and material collected by the U.S. Strategic Bombing Survey.

Parillo's work also demonstrates, however, that the Japanese failure was not simply one of practice but also one of policy. As a matter of policy, the Japanese allocated too many resources to building giant warships like the Yamato class battleships and too few to merchant shipping and antisubmarine vessels. It was not simply Japan's antisubmarine efforts that were flawed, but their entire logistical system. Even during the campaign for Guadalcanal, when American submarine efforts were still negligible and Japan's navy roughly equal to the U.S. fleet in the Pacific, the Japanese still managed to supply their forces on the island with only one-tenth of the material reaching the Americans. By highlighting the logistical dimension of the conflict, Parillo's book provides an important and needed new perspective on the war with Japan.

RONALD SPECTOR
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BYRON K. MARSHALL. *Academic Freedom and the Japanese Imperial University, 1868–1939*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xiv, 247. \$40.00.

Which chunk of the vast stretch of time historians carve for their study depends on their originality. Despite this book's title, Byron K. Marshall chose the basic time frame of the period 1905–39, a decision that helps him shed new interpretive light on familiar materials and make his book thought provoking. In 1905 and 1939, similar incidents occurred at Tokyo Imperial University. They were similar in the sense that in both years the Japanese government threatened the freedom of speech of the university's faculty and its self-rule. In 1905, however, the university successfully warded off the threat, whereas in 1939 it succumbed and sacrificed individual faculty members, to the point of terminating their employment, in exchange for the safety of the institution's self-governance. What accounted for this difference?

Marshall suggests a number of factors: the educational and career backgrounds of the professors of imperial universities on one hand and politicians and bureaucrats on the other; the sheer size of the universities; the loci of knowledge and skills needed to build and maintain a strong Japan; ideologies. In the late-Meiji period, for example, professors, politicians, and bureaucrats not only came with some shared backgrounds but also constituted interchangeable parts of the machine that ruled Japan. Conflicts among these parts could be, and had to be, resolved without creating irreconcilable rifts. Three decades later, the Japanese government no longer depended on the expertise of academic intellectuals, so that it could act less prudently in dispensing with their security and rights. These arguments of Marshall's are insightful and persuasive.

One is also easily convinced of the evil of factionalism that Marshall emphasizes as having caused the colossal failure of university faculties, especially the economics department of Tokyo University, when they were confronted with government censures in the hard times of the 1930s. It was indeed tragic for the individual faculty members to be engulfed in factional strife, since they were genuinely committed to the ideologies that divided men who might otherwise have been friends. Their demise was even more tragic, for, in the vital instance, their colleagues elected a pro-government president, a former naval engineer, in the name of preserving the university's institutional integrity. No doubt in those days as today, too many individuals hid themselves behind "the good of institution" to salvage their own personal gains.

What might be added to this admirably concise work even at the risk of causing tedium is a more detailed study of the social, geographical, and generational diversity of the innumerable individuals identified by name as the key players in the episodes of

1905 and the 1930s. Differences in class or status between those involved in the affair of 1905 and their counterparts of the 1930s seem significant. No matter how impoverished, the former belonged to the samurai ruling class, while the same cannot be said of the younger people. From which domain of the Tokugawa period any given Japanese came influenced his or her career course and political position far more before the war than today. The key players in the 1905 conflict were close in age as well as outlook, but by the 1930s there were "older" university administrators making decisions that affected the "younger" faculty with vastly different views of the world. In aggregate, these factors probably made much difference in the outcomes of the cases compared.

One small caveat may be added in closing. Throughout the book, Marshall refers to Tokyo Imperial University as "*Todai*," which in fact is a misnomer. Both as an institution and a nomenclature, "*Todai*" is a postwar product. Properly speaking, "imperial university" (*teikoku daigaku*) is abbreviated as "*teidai*," and, if no special mention is made, "*Teidai*" is understood to mean Tokyo Imperial University. This little footnote may be taken as friendly advice because there still are stalwarts in Japan who refuse to call their alma mater by its postwar nickname, and who rightfully reject the notion that *Teidai* looked anything like its postwar incarnation.

ATSUKO HIRAI
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MARTINA DEUCHLER. *The Confucian Transformation of Korea: A Study of Society and Ideology*. (Harvard-Yenching Institute Monograph Series, number 36.) Cambridge: Harvard University Press. 1992. Pp. xiii, 439. \$35.00.

This is an epoch-making book for Asian studies, and for Korean studies in particular. Students of China and Japan will learn the rich and unique experience of Neo-Confucian vision and adaptation in dynastic Korea. Scholars of Korea owe a debt of gratitude to Martina Deuchler for her erudite and milestone study of the pre-Confucian Koryō and early Chosōn society and its gradual transformation under the state orthodoxy of Neo-Confucianism. The book is superbly researched and minutely crafted, incorporating the major findings of specialists in the field and providing a historical perspective that is comparative, multidisciplinary, and stimulating.

The book is set up to "test two major hypotheses" (p. 6): first, that late Chosōn society after the second half of the seventeenth century was a stark contrast to the Koryō and early Chosōn society that preceded it; and second, that Neo-Confucianism was the driving force for this change. These are familiar hypotheses to researchers in Korean social history, but Deuchler succeeds in reconstructing the process of change and continuity more eloquently, thoughtfully, and com-

paratively than anyone in the field. She has marshaled critical evidence from the dynastic annals, legal codes, literary collections, and other historiographic records. The book will remain as a basic guide and lofty standard for studies in the social and intellectual history of Korea and comparative cultural studies of East Asia, including women's studies.

Deuchler defines at the outset two basic focuses: that the lineages (*yangban*) represented the social organization of the Korean upper class (p. 12); and that Neo-Confucianism was the ideology of change through rites and rituals, as well as through a centrally organized state and through education and morality (pp. 15, 27). Chapter 1 discusses the unique features of Koryŏ society in terms of kinship, succession and inheritance, marriage, and mourning rites. Contrary to the Confucian system, a Koryŏ kinship group was inclusive and multilateral: kin consisted of patrilineal, matrilineal, and affinal relatives without patrilineal bias (p. 38). Descent was not traced unilineally, and a person's social status was established by bilateral consideration. All members of the same generation, male and female, enjoyed equal rights and duties (p. 45). Thus, succession was flexible and non-linear; brothers and sisters were co-heirs. Primogeniture was absent (p. 47). Marriages with close cognates were customary. Uxilocal residence was typical for the newly married couple: the bridegroom moved into the bride's house and their children, and often grandchildren, were born and raised in the mother's house (p. 66). Deuchler finds similarities between the Koryŏ society and that of Heian Japan (p. 82). She correctly observes that "the most crucial . . . element of high status was descent" (p. 86).

Chapter 2 provides historical background for the social legislation of the founding elite of the new dynasty, whose vision for social transformation originated in Neo-Confucianism. They believed in Confucian morality and that "law and institutions were a visible manifestation of morality." Deuchler refers to Chŏng To-jŏn's *Code of Administration of the Chosŏn Dynasty* (1394), Cho Chun's *The Six Codes of Administration* (1397), Ha Yun's *Amended Six Codes* (1413), and *Great Code of Administration* (1471) (pp. 120–22). The striking feature of these founding elite forces was that they were not new social forces but rather were represented descendants of well-established aristocratic kin groups from the Koryŏ.

The next four chapters, the substantive part of the book, shed light on the problem of change and continuity in Korean adaptation of agnation and ancestor worship, mourning and funeral rites, inheritance, and Confucian legislation and the consequences for women. Deuchler argues conclusively that the Korean experience of Confucian transformation "represented a uniquely Korean interpretation and adaptation of the Confucian model" (p. 289). The Korean Confucians "drew strength from its [status consciousness] and modified the Confucian

message in crucial areas to fit it into their country's social tradition" (p. 290). To Deuchler, Koryŏ aristocratic tradition was incorporated into the new Confucian order in the name of "national practice" (*kuk-sok*) (p. 125).

Deuchler explains why and how Neo-Confucians attempted to establish agnatic lineal descent groups through the mechanism of ancestor worship and ritual primogeniture by the principle of direct lineal descent. Despite legislation in the fifteenth century, however, it took nearly two more centuries for the agnatic lineal descent groups to develop fully in Korea. Negative consequences of this development became evident in the status of second sons and women, who came to lose their customary equality and advantages. Confucian vision was further legislated in the new systems of mourning and funeral rites, inheritance, and wedding ceremonies. The general trend was toward a more rigidly defined hierarchical reclassification of Korean society based on Chinese classical literature. Equal inheritance among sons and daughters was challenged by the emerging concept of patriliney and the principle of agnatic succession, a shift from horizontal to vertical channels (p. 224).

One of the fascinating points Deuchler makes is the effect of Confucian legislation on the place of women. Neo-Confucians were preoccupied with the "rectification of names . . . with determining and identifying social statuses" (p. 232). By distinguishing primary and secondary wives, they attempted to establish a "clear line of descent" that would "provide the criteria on the basis of which descent group membership and thus social status could be verified" (p. 232). The *sadaebu* (the scholar-official class of the Chosŏn dynasty) was allowed to have only one legal wife (p. 234). Eventually primary wives came from the *yangban* elite, and secondary wives from the lower classes. This ranking of wives contributed to the division of society into noble and base categories, making women "all the more key figures in transmitting the rights to descent group membership and the keepers as well as victims of an unequal system" (p. 236).

Deuchler is right in concluding that descent and heredity are critical for the status of the *sadaebu*, who belonged to a socially homogeneous and practically endogamous group (p. 298). Their membership was basically determined by birth. The society of Chosŏn Korea was perceived as forming two major groups, largely defined in terms of purity: the noble and the base (p. 302).

Thanks to Deuchler, traditional Korean society is no longer an unknown entity for students of East Asian history. The book leaves us with stimulating and exciting thoughts about the Korean culture before, during, and after its Confucian transformation.

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KENNETH M. WELLS. *New God, New Nation: Protestants and Self-Reconstruction; Nationalism in Korea, 1896–1937*. Honolulu: University of Hawaii Press. 1990. Pp. xi, 222. \$32.00.

Korea is unique in Asia for its large and influential population of Protestant Christians who today comprise more than a fifth of the total population of the Republic of Korea. The phenomenal success of Protestantism in Korea is related to the number and prominence of Protestants in Korea's modern nationalist movement, but the political activities of these figures have usually been considered outside the context of their religious beliefs. Kenneth M. Wells bridges this gap by noting that a large and widely influential group of Protestant nationalists had an ethical notion of reform of the Korean nation based on belief in an "intrinsic connection between Christianity, modern education and national revival" (p. 30).

What Wells terms "self-reconstruction nationalism" (pp. 9–10) involves separating the idea of nation from that of state and attributing Korea's problems to a national culture that lacks an altruistic public morality. Self-reconstruction nationalists felt Korea's renewal could be built only on new personal morality, impossible in practice without the help of Christianity. Wells illustrates this thesis with a chronological account, focusing particularly on Yun Ch'i-ho (1865–1945), An Ch'ang-ho (1878–1938), and Cho Man-sik (1882–1950), whose activities stemmed from a conviction that national renewal depended on an indispensable foundation of education and cultural development without which political activity would be useless.

The notion of self-reconstruction nationalism works well for Yun Ch'i-ho, whose voluminous English-language diaries give Wells the material he needs to connect his political activity with Christian morality and Protestant discourse on the relationship of state, religion, and society. It is a more problematic notion for the later figures, for whom Wells less convincingly makes the link between social ideology, religious conviction, and Protestant discourse. After 1919, when nationwide demonstrations against Japanese rule began to mobilize large segments of the Korean population, the Russian Revolution provided a competing model for national reconstruction. The failure of Western Christian nations to support Wilsonian notions of self-determination, moreover, led to widespread disillusion with the West and Christianity. By this time, a notion of national reconstruction that transcended questions of class structure and political organization (as with the biblical injunction to "render unto Caesar") seemed inadequate to many people, and Wells's notion of self-reconstruction nationalism becomes a procrustean bed for dealing even with Protestant figures.

Wells's book is an interesting, original, and solidly researched contribution to our understanding of Christianity and Korean nationalism. As with many

good books, he raises more questions than he can answer. His promising notion that a universal religion and nationalism make uneasy bedfellows remains underdeveloped. One wonders how figures other than Yun reconciled widespread Spencerian notions of social evolution with conservative Protestant theology, and if the notion of social gospel was not also important. One also wonders how addition of the politically important Christian socialists who emerged after 1919, such as Yŏ Un-hyŏng or Kim Kyu-sik, would affect Wells's analysis.

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JOHN A. LARKIN. *Sugar and the Origins of Modern Philippine Society*. Berkeley and Los Angeles: University of California Press. 1993. Pp. xvi, 337. \$48.00.

John A. Larkin's long-awaited study of the place that sugar occupies in the failure to develop a well-rounded Philippine society is a tour de force. He tells us that his treatise is neither national nor regional but rather comparative and integrative. His study is a complex examination of an industry that has held a cruel grip over the lives of many Filipinos, rich and poor, for several centuries, especially since the turn of this century.

Larkin's monograph is divided into six chapters of unequal length covering the entire span of modern Philippine society. His first chapter joins sociology, economics, and political science with history in order to introduce his subject.

Chapter 2 traces the slow social and economic development of the indigenous elite in Pampanga and Negros and their collaboration with Spanish authority and the role that sugar would play in enhancing that relationship. The slow growth of the sugar industry over the course of nearly 300 years was due in part to the lack of Spanish interest and almost nonexistent world-wide demand for the commodity. The Industrial Revolution and Spain's need to make the Philippines economically viable, however, combined to push sugar to the forefront in Pampanga and Negros, where soil and social climate made large-scale production possible.

The next chapter examines the rapid growth of the sugar industry in Negros as world demand for the commodity made hacienda development an attractive means for a small group of indigenous elite and some outsiders to enrich themselves quickly. In Pampanga, sugar production was not as rapid. The local elite chose tenant farming as the primary means to grow sugar. World-wide demand, particularly during World War I, led to overexpansion and collapse following the war's end.

Chapter 4 looks at the character and nature of provincial society and, to a lesser degree, high society in Manila. It was boom and bust for the powerful and grinding misery for the migrant sugar workers in

Negros and constant hard times for the tenant farmers in Pampanga. Larkin uses this chapter to lead us to the final two chapters: sugar and its place in the high stakes of Manila-centered politics, and the rude awakening after 1935 that sugar, which dominated Philippine trade with the United States, was no longer king.

The years between 1920 and 1934 were hard for sugar growers and those who worked the fields, while for the sugar centrals the times were good. The misery of those who did the sweat labor, especially in Pampanga, led to the rise of demands for radical solutions, such as joining the Socialist or Communist parties.

The establishment of quotas for sugar after 1935 meant adjusting to the end of sugar as the means for creating a favorable balance of trade for the Philippines. The attempts of the sugar industry to reverse the course of Philippine independence was not supported by young idealists having no connection with sugar.

In his epilogue, Larkin wonders if the successors of President Corazón Aquino will have the wisdom to alter the pernicious stranglehold that sugar has held over Philippine society since the 1880s. We have our doubts.

Larkin's editor should have spent money for a better index and a standard bibliography, and should have checked for errors in names and dates. But these small irritations caused by penny-pinching can be overlooked when placed against this important contribution to Philippine studies.

MICHAEL PAUL ONORATO
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GEORGE D. WINIUS and MARCUS P. M. VINK. *The Merchant-Warrior Pacified: The VOC (The Dutch East India Company) and Its Changing Political Economy in India*. New York: Oxford University Press. 1992. Pp. viii, 201. \$15.95.

Few of the Dutch remember today that their forebears had a very formidable presence in the Indian subcontinent for nearly two centuries, primarily during the seventeenth century. Likewise, few historians are aware of this Dutch presence in India, remembering primarily the celebrated victory of the English over the French during the eighteenth century and the subsequent creation of the colony of British India. The historical preoccupation with Dutch control over Indonesia (Dutch East India) has so overshadowed Dutch activities in India, Ceylon, and Bangladesh that the latter has almost been forgotten. George D. Winus and Marcus P. M. Vink have written a valuable reminder of that nearly forgotten chapter in the history of European expansion.

This book is the outgrowth of a series of three lectures—the Henry Heras Memorial Lectures—de-

livered in 1988. As such, it is brief and does not present a comprehensive history of the Dutch in the Indian subcontinent. Dutch activities in the various regions of India have been studied and written about by Indian scholars. This book summarizes that work, and, by adding some new data, it provides a useful synthesis of the overall Dutch presence in that region.

The book is divided into three chronological chapters, each representing a period or phase of the Dutch presence in India: the monopolistic phase (1600–80), the competitive phase (1680–1748), and the period of disengagement and decline (1748–95). Each chapter starts with a general survey of the period, stressing the Dutch activities in relation to their European rivals. Subsequently, the various regions in India where the Dutch had a commanding presence are examined in respect to relations with Indian leaders and populations as well as to relations with non-Indian competitors. The regions include the northeastern area of Bengal, the northwestern region of Gujarat and the port of Surat, the southwest Indian coast of Malabar, the southeast coastal region of Coromandel, and, finally, the island of Ceylon (today's Sri Lanka). Each chapter heading reflects the theme well, except that in the first phase the term "expansion" or "conquest" might well be added to the term "monopolistic."

Winus and Vink stress that the Dutch activities in the Indian subcontinent were essentially commercial, *emporalism* rather than imperialism. Whereas the Dutch became imperialists elsewhere, in India and Sri Lanka they limited themselves to the strategy of *emporalism*, according to the authors.

The book tries to demonstrate the interconnectedness of the Asian trade under European supervision. It also suggests that, for the Dutch, India was as important as the East Indies during the second half of the seventeenth century, and that Ceylon was a serious rival of Java for the headquarters of Dutch activities in Asia. The book tends to be more sympathetic to the Dutch than to their Asian and European competitors.

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SUDHIR CHANDRA. *The Oppressive Present: Literature and Social Consciousness in Colonial India*. New York: Oxford University Press. 1992. Pp. xi, 192. \$19.95.

This is a very intelligent book. It is not an easy read, but Sudhir Chandra rewards the careful reader with his cultural sensitivity and attention to detail. Chandra seeks to understand the dominant structures of social consciousness in modern India. His quest leads through a nuanced reading of late-nineteenth-century literature in several North Indian languages—Hindi, Bengali, Gujarati, and Marathi—by Hindu men of letters whose education and experience brought them into contact with the British colonial

presence. The linguistic representativeness is impressive, as is the range of literary genres covered. Chandra searches the collected works of his chosen authors seeking social reform tracts, literary journals, novels, and autobiographies that expound recurring themes. Such themes include the attempt to come to terms with the loss of power, the search for a sense of cultural worth, and the quest for a usable past.

Such a bald summary does little justice to the complexity of Chandra's argument and the care with which he selects and explicates his examples. The authors he covers include Bharatendu Harishchandra, Bankimchandra Chatterji, R. C. Dutt, V. K. Chiplunkar, Govardhanram Tripathi, and a host of others. Chandra points to the paradoxical tendency on the part of his authors to express gratitude for the benefits of British rule while chafing under their situation of dependency. This intellectual tension could be rationalized by an appeal to a mythologized past. British rule might be oppressive, but—according to their colonial textbooks—the British had rescued them from degradation and anarchy. The cause of that social and political decline from a golden age of the past was explicable. Various authors attributed any Hindu custom viewed as backward in the Victorian era—caste distinctions, widow-burning, child marriage, and purdah—to that earlier and longer bout of alien rule by Muslims. Such an explanation made it possible to be loyal to the British and still salvage Hindu self-respect.

The cognitive dissonance of the Hindu literate elite is a theme that runs through Chandra's subsequent analysis of their attitudes concerning family and tradition: how social reformers preached individualism and women's rights but succumbed to family pressures in practice. In the development of a nationalist consciousness as well, those intellectuals who most valued national unity in the quest for independence paradoxically treated Muslims, no matter how Indian their lineage, as aliens. Indians were Hindus.

There is much more to this tightly packed, carefully researched work than can be covered in a short review. It is not flawless. Toward the end of the study, Chandra devotes several pages to a discussion of regional linguistic identities, but these ideas are inadequately developed and seem extraneous. The book lacks a bibliography, which is a serious omission. On balance, however, Chandra succeeds in his expressed desire to achieve some critical empathy for his subjects.

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JAN KOCIUMBAS. *The Oxford History of Australia*. Volume 2, 1770–1860, *Possessions*. New York: Oxford University Press. 1992. Pp. xv, 397. \$45.00.

The bicentennial of white settlement in Australia in 1988 was the occasion for a number of important projects aimed at reassessing Australian history. *The Oxford History of Australia*, under the general editorship of Geoffrey Bolton, was conceived in this way, but unlike the ten volumes of *The Australians: A Historical Library*, and the four volumes of *A People's History of Australia*, both of which are collected works with a wide variety of contributors writing short, specialized pieces, the five volumes of *The Oxford History* are single-author volumes, allowing the five chosen authors greater scope to offer individual interpretations of their allotted timespan.

The advantage of this approach is that it allows the individual author a greater expansiveness, an opportunity to impose a broader structure on the argument, to cross-reference across chapters, and to allow themes to emerge naturally through the narrative. It also allows an individual to impose his or her idiosyncrasies and crotchets on current historical debates from the authoritative position of one of the authors of *The Oxford History of Australia*. This may or may not be a good thing; it certainly makes for a more interesting and entertaining read than the sometimes bland offerings of multi-authored works.

A disadvantage of relying on single authors, however, lies in the vicissitudes associated with meeting deadlines. This bicentennial project faltered, at least as a bicentennial project, when, for a variety of reasons, not all five volumes were published by 1988. Jan Kociumbas's contribution was published in 1992. Covering the period from the beginning of white settlement in 1770 until 1860, it was planned as the second volume of the series. The promised first volume, *Aboriginal Australia*, by Tim Murray, has still not appeared.

This background highlights a problem in reviewing Kociumbas's book for, while it stands alone as an excellent introductory history of early colonial Australia, it must also be seen as part of a still-incomplete series. The knowledge that *Aboriginal Australia* would be covered by someone else must have affected Kociumbas's choice of subject matter, for she would have known that one part of her task, the explanation of the Aboriginal context within which white settlement occurred, was shared with another volume. Hence an apparent gap in her study for *Terra Nullius*, the legal fiction that Australia was unoccupied, and which allowed Britain to take possession of the continent by right of occupation without reference to its prior inhabitants, is dealt with surprisingly fleetingly in a book entitled *Possessions*, and does not appear in the index. (There is a critical line missing from footnote 21, p. 346, which must refer to the decision of Mr. Justice Blackburn on *Terra Nullius*.) This lacuna is the more unfortunate because, as a general history of colonial Australia from 1788 to 1860, Kociumbas's book is impressive.

The book begins with an account of the flogging of three women convicts in 1791, under the order of the

diarist, Lieutenant Ralph Clark. Catherine White fainted after the first fifteen lashes, Mary Teut after twenty-two, while Mary Higgins received twenty-six, after which Clark "forgave her the remainder, as being an old woman." The floggings "will be a warning to the ladies," Clark noted. Much later in the chapter, we learn that, at this time, another of the convict women was already six months pregnant, with Clark's child—a fact he never confided to his diary.

Here we have, in miniature, many of the themes that recur in Kociumbas's book: the brutality of power and the ambiguity and hypocrisy of an administration that simultaneously punished and exploited women for their sexuality. From this particular incident, Kociumbas moves to the general picture, looking at the sexual and reproductive roles of women within the settlement and relating these back to the ideological assumptions of eighteenth-century British colonial policy, which required women to pacify and domesticate male violence and to reproduce a work force. Kociumbas is now working on a history of childhood in Australia, and she has much to say about the experience of children in the early colony. Her policy of writing about women and children first effectively subverts the established order; most historians have not prioritized their experience in this way.

Later chapters take up the theme of "Possessions," the subtitle of the book. Chapter 2 is entitled "Thirty Acres," a reference to the small blocks of land that the British government granted to male ex-convicts in New South Wales. These grants marked a stage of small-scale agriculture that was soon displaced by the growth of large-scale pastoral capitalism. Kociumbas's theme is that colonial settlement occurred against a background of the polarization of merchant and industrial capitalism in Britain and that subsequent colonial development was shaped by the needs of industrial capitalism in Britain. As these needs changed, colonial policy was adjusted in line with British requirements, with the later creation of new, non-convict colonies in Western Australia and South Australia in part reflecting that shift. This is not a particularly profound insight, but she argues the case effectively. She also manages to deal with the narrative problems that arise from the need to handle the history of six different colonies, which were established at different times, for different purposes.

One of Kociumbas's greatest strengths is as a narrative writer. People as varied as the hypocritical Ralph Clark, Pemulwuy the Aboriginal resistance leader, and Elizabeth Needham, the convict woman who survived a beating by one of the marines to become one of the first women landowners, emerge as vivid, if fleeting, individuals.

This study is based on published sources, including such substantial documentary collections as *Historical Records of Australia*, as well as the broad range of writing on Australian history that was both celebrated and stimulated by the bicentennial. As such, Kocium-

bas's book is a highly successful work of synthesis. It is a pity, however, that her historiographical assessments, together with some of her more general arguments, have been relegated to the footnotes. Perhaps it would have disrupted the vigor of her narrative to bring these discursive pieces into the body of her work, but readers should at least be warned that some of the best bits are in the footnotes.

MARION DIAMOND

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JAMES A. GILLESPIE. *The Price of Health: Australian Governments and Medical Politics, 1910–1960*. (Studies in Australian History.) New York: Cambridge University Press. 1991. Pp. xvii, 358. \$59.50.

The health-care policy debate in Australia has a lengthy and often strained history. Arguments have reflected familiar efforts at balancing quality, accessibility, and affordability. In his dense, scrupulously researched study, James A. Gillespie is equally if not more concerned with political dynamics than with substantive issues. Health care/medical politics is an uncommonly handy vehicle for assessing public values and expectations, lobby politics, party politics, and decision-making structures and styles that inform the national scene.

Explicitly or by inference, an Australian political profile emerges, with particular emphasis on the "politics of access" to the political system and on the control of medical services—a variation on "who gets what, when, and how."

Among the book's recurrent themes is the medical profession's efforts to protect its own independence, its fee for service preference, and other interests. As has often been noted about the Australian idiom, however, a tradition of aid for the needy, of a measure of social equity, and of "fair go" has mediated, or tempered, class divisions and narrowed attitudinal gulfs. The Australian branch of the British Medical Association often resisted government attempts to impose uniformity and control over health care, but not blindly. Tactical considerations played a part, as did cultural norms and considerations of professional responsibility. Hence the medical profession's occasional administrative noncooperation in contentious regulations did not approach actual threats to withdraw patient services. Medical profession efforts at exerting effective pressure at the national level were in turn handicapped by entrenched, state-based differences in organization, leadership, and frequently in outlook.

Constitutionally enshrined federalism and its attendant politics inhibited the development of clear and comprehensive national health-care strategies. Outcomes were not, however, entirely without redeeming features. Whereas the federal Department of Health struggled for years with marginal resources and a

modest brief, hospital and administrative health-care reforms in Queensland for some time offered progressive models.

The Australian public's own sense of fair play and compassion for the underdog was de facto partially offset by suspicion of political authority. This made it more difficult for federal Labour governments of the 1940s to mobilize opinion behind ambitious health-care schemes. Also to blame, however, were flawed political judgment, legal obstacles, fiscal caution among Treasury bureaucrats, and medical profession lobbying. But at various stages of Australian policy debates, the medical profession was not the only complaining constituency. Trade unions could be found embracing principles of medical insurance coverage while resisting funding strategies. The Catholic hierarchy deplored the greed of the medical profession, but in the climate of the late 1940s Catholics voiced alarm over dangers posed to the family by a social-service state.

After Labour lost federal office in 1949, its Conservative Party successors were not content to follow medical care *laissez faire*. They contributed the foundation on which later versions of a national system were built. The weak and dependent were to be sheltered, but benefits were not denied to the middle class.

For a time in the interwar period, rationales guiding officially promoted "public hygiene" were not simply aimed at health. The nurturing of physical well-being and strength was associated with celebrating a racially and ethnically homogeneous community. Gillespie's account stops in 1960. Succeeding decades have produced deep social and economic transformations. Australia now is more diverse, open, and innovative, yet its people still argue over features of health-care policy. But the broad framework of approved public sector engagement in the health-care field has shifted; more is expected, and more is being done, while health-care providers maintain standards of excellence.

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KLAUS NEUMANN. *Not the Way It Really Was: Constructing the Tolai Past*. (Pacific Islands Monograph Series, number 10.) Honolulu: University of Hawaii Press. 1992. Pp. xvi, 310. \$34.00.

The Tolai people of the island of New Britain number some 80,000, making them one of the largest ethnic groups in Papua New Guinea; they also were among the earliest to experience sustained contact with Europeans, beginning in the 1870s. Travelers, missionaries, government officials, and ethnographers have produced a written record that is unusually extensive for this part of the world and, as Klaus Neumann demonstrates in his book, oral histories

(accounts of events witnessed by the narrator) and oral traditions can add richly to our understanding of the colonial past.

But Neumann, who supplemented his library and archival research with nearly two years of fieldwork in Tolai communities, apparently is not interested in a synthesis or reconciliation of these diverse sources, which often differ considerably with respect to particular events. Citing Walter Benjamin as a major influence, Neumann's goal is "a construction . . . of the past in opposition to a historicist reconstruction" (p. 42). He rejects what he sees as the conventional historian's quest for objectivity and factual accuracy: trying to determine "the way it really was." Indeed, for Neumann's history "it is irrelevant whether the events reported in the [Tolai stories] . . . 'really' happened" (pp. 60–61). He is more interested in how the past is "resurrected" through conventions of "social frames of the present" (p. 241), usually for political purposes, for example, to justify claims of land ownership. Adopting the "montage method" of German film director Alexander Kluge, Neumann experiments with diverse presentation styles, eschewing any attempt at chronological order and alternating chapters that set out his reflections on historiography and the cultural and political contexts in which Tolai produce their "histories" with chapters that comprise Tolai-originated versions of particular (purported) events.

If there are still historians who need convincing that any representation is a motivated one, or that our understanding is always partial (in both senses of the word, as Neumann would have it), then the "Tolai histories" rendered here, together with Neumann's careful exposition of some of the subjective factors that influenced their construction, constitute a series of object lessons. But Neumann's goal is not simply illustrative; in frequently strident tones, he poses as a revolutionary, "encircling hegemonical History with a colorful diversity of counterhegemonical histories" (p. 259), the better, it would seem, to subvert "it." What is not explained is how the Tolai, for whom Neumann clearly feels deeply, have been disserved by the "hegemonical History" produced by conventional scholars. Nor is it clear how his offering is an improvement, for it is not simply a matter of privileging "the Tolai version" of events, since none exists. Instead, "Tolai histories differ depending on who tells them, when, and where" (p. 260), and Neumann's narrators, mostly older men from coastal villages, "did not represent a cross section of the community" (p. 7). As the largely literate Tolai work at reinventing *kastom* and traditions to define themselves in the modern world-system, one wonders what they will make of this, inevitably idiosyncratic, "construction" of their past.

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MARSHALL SAHLINS. *Anahulu: The Anthropology of History in the Kingdom of Hawaii*. Volume 1, *Historical Ethnography*. Assisted by DOROTHY B. BARRÈRE. Chicago: University of Chicago Press. 1992. Pp. ix, 243. \$50.00.

PATRICK V. KIRCH. *Anahulu: The Anthropology of History in the Kingdom of Hawaii*. Volume 2, *The Archaeology of History*. Chicago: University of Chicago Press. 1992. Pp. xiv, 201. \$50.00.

Marshall Sahlins and Patrick V. Kirch have written an unconventional history, presented in two volumes that look unconventional: each is larger than an ordinary text to allow for photographs, maps, and charts (themselves quite wonderful). The text is arranged, rather awkwardly, in two columns on each page. The theoretical framework is also startling; this is history from the ground up in several senses. Not only are the documentary records grounded in the archaeological evidence but also the history of Hawaii's entry into a world system is based on the actions and statements of Hawaiian people ranging from the chiefs who met the conquerors to the people who asked for their land in the *Mahele*, the great land distribution of 1846–55.

As Sahlins and Kirch write: "Our aim is to bring down the World System, through the mediation of the Hawaiian system, into the way the rock walls are laid out, the remains of houses show forth, and the irrigation ditches are traced in the soil of the upper Anahulu River Valley" (vol. 1, p. 101). The authors restate this goal at various points so that the reader comes to realize the complexity of the task they have set themselves. The agenda includes not just rewriting history, but redefining history as a discipline; in counterpart, the discipline of archaeology is challenged and revised under the aegis of powerful archival evidence. The two volumes lean on but do not absolutely need each other. Separately, each presents a wealth of material and innovative interpretations of Hawaiian life from before contact until the mid-nineteenth century. Together, the two volumes compel a reconsideration of interdisciplinarity and, by making the reader work, recall how easily we are lulled by certain styles of historical writing.

Why Anahulu, a river valley in northwestern O'ahu? Partly by plan and partly by the chance of a good excavation site. The plan was to use a small area to illustrate large forces: Anahulu contains in "miniature, almost in domestic detail, the main historic relationships" (vol. 1, p. 186). The effort to discover in local arrangements the "lineaments of a greater history" (vol. 1, p. 143) is a mixed success: volume 1 tends to lose Anahulu in the broader story of Hawaiian engagement with a world system, while volume 2 forefronts the locality to the exclusion of background.

The stated intention, however, allows the authors to reverse the usual versions of Hawaiian history, which turn out to be Western history with Hawaii included. Captain James Cook makes only a brief

appearance; the governments of the United States, England, and France barely exist; and the action is created by chiefs with large appetites rather than by missionaries or diplomats with overweening ambitions. Readers, in fact, are warned by an epigraph from Voltaire that they will be "obliged to struggle against boredom to go through all the particulars of obscure calamities confined to a little corner of the world." This is, I assume, ironic; Sahlins and Kirch do not mean us to be bored but rather to discard our complacent (and condescending) assumptions about what history is. History, we realize as we struggle with unfamiliar characters and strangely motivated crises, has had a dominant Western-based narrative. These volumes topple that narrative in their detail and in their appearance.

Yet nothing is claimed without the standard evidence a historian—and an anthropologist, for that matter—would require. Sahlins and Kirch draw on a wealth of material, published and unpublished, written and nonwritten. They modestly credit the interdisciplinary nature of the inquiry for imaginative readings that one suspects each was primed to make (both authors have been at this revisionary work for a long time.) In volume 1, we learn about the sources, themselves historicized and thus inevitably "distortions" of "reality" (vol. 1, p. 4). For the historical ethnography, in addition to the usual journals, diaries, letters, and official documents, there are (for instance) account books, lists of ships and weights, and the wonderfully fruitful letters submitted to the Land Commission. These letters frame the archaeological analysis, and Kirch appropriately uses the terms *mauka* (landward) and *makai* (seaward), *Ko'olau* and *Wai'anae* (neighboring districts) as directional coordinates (vol. 2, p. 24).

Throughout, we hear the voices of individuals (162 letters were sent to the Commission from Anahulu [vol. 1, p. 14]), a necessary concomitant of the central thesis: "All this means that Hawaiians too were authors of their history and not merely its victims" (vol. 1, p. 215). As with other claims in the two volumes, this sounds deceptively simple. Sahlins and Kirch, each differently, demonstrate the complexity of the "authorship" of Hawaiian history: the changing distribution of power, the inevitable factions on all sides, and the eccentricities of a trade that included everything from sex to sandalwood, from *poi* to Bibles. The unmistakable point is that the individuals who make history (and it is individuals who do) are driven by greed, competitiveness, desire, and love of the land.

Individuals are essential to the Hawaiian system that mediates the world system (vol. 1, p. 2). System is a crucial word, and applied to Hawaii blurs with the concept of structure Sahlins developed in *Islands of History* (1985). Related to the historian's *longue durée* and the anthropologist's culture, structure suggests the persisting patterns on which events impinge and through which they are transformed. The concept drives interpretations in both volumes. In *Historical*

Ethnography, for instance, Sahlins shows how the traditional Makahiki festival structured Hawaiian resistance to the onslaught of foreigners through the first half of the nineteenth century; these outbreaks of *haunaele* (commotion) and lewd behavior became less successful as Christianity and capitalism became competing structures. For the archaeologist, too, the concept of structure has resonant meaning, from layers of evidence to the shape of a social polity that makes sense of material artifacts. Here the two volumes come together most effectively, as Kirch's descriptions of transformations in space reflect the shifts in rule that Sahlins describes.

The two also come together in their deep appreciation of the struggle of the Hawaiian people, from the "consuming" chiefs to the "consumed" commoners, to survive. But volume 1 is the tragedy, an account of the rhythms of history whose only climax can be loss. The inevitability is at once characterological—dealing with the personnel of history—and systemic, relating the sweep of capitalism. Volume 2 sounds a more positive note, perhaps because it rests on what has lasted rather than on what has disappeared. Yet neither volume, filled with individuals who may have authored but could not control their fates, ends happily. In each version of history, the *maka'aina*, the people of the land, are dispossessed.

At the same time, one cannot finish reading the two volumes without being exhilarated by what was attempted. This is an exciting collaboration precisely because it challenges each discipline: archaeology is not the same after this work, nor ought history be. After reading the two volumes, one must accept the multivocality of history. As fluid as the Hawaiian kinship system, the word refers to: events patterned by a structure; narratives variously constructed; the metaphors through which realities are transmitted; and the layered landscape of change. My historian colleagues occasionally suspect that an anthropology of history makes history disappear. On the contrary, as these volumes prove, anthropology makes history the more significant by locating it in the slightest actions of the least important people in the farthest corners of the world as well as in the actions of the mighty who stand on Europe's center stage.

A review of even a thousand words cannot do justice to the complicated picture Sahlins and Kirch paint. Moreover, my own perspective of the cultural anthropologist is itself only one take on the arguments and the evidence presented. Historians and archaeologists would review this work differently. In the end, too, it is we specialists who will be the most avid readers of these volumes. The books have not been written for a lay audience. The challenge is posed to those who are comfortably, and disciplinarily, trained; it is up to them to take the lessons of this work to a broader audience.

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ELIZABETH BUCK. *Paradise Remade: The Politics of Culture and History in Hawai'i*. Philadelphia: Temple University Press. 1993. Pp. viii, 242. \$34.95.

Recognizing a relationship between music and culture, Elizabeth Buck examines the evolution of Hawaiian music as a way of understanding Hawaii's cultural past. In this small but thoughtful book, the author synthesizes the ideas of Louis Althusser, Ferdinand de Saussure, Michel Foucault, Karl Marx, and others to define music's role in two centuries of Hawaiian cultural transformation and, in effect, create an alternative history of the islands.

After a discussion of Marxist theory, Buck focuses on the precontact Hawaiian economy and social structure. She then carefully reviews the economic shift of the islands into a capitalist system and the cultural disharmony caused by increasing European presence. In the last chapter she compares the fate of Hawaiians under the "colonial" capitalism of the sugar industry with the "corporate" capitalism of the tourist industry.

Buck weaves the development of Hawaiian music into this history. Prior to contact, most formal Hawaiian learning, including cosmology, history, and genealogy, was stored within the chant and hula and recited in ceremony by the Hawaiian nobility (*ali'i*). In 1819, after four decades of European influence, Liholiho abolished the ancient *kapu* system. Without the restrictions imposed by *kapu*, new and less demanding forms of chant and hula evolved that diminished the traditional importance of the older rituals. Several years later the Protestant church morally condemned chant and hula and, thereafter, newly christianized Hawaiians lost interest in them. The missionaries' successful introduction of writing and English also reduced demand for the memorized cultural knowledge of the *ali'i*.

Much of the traditional Hawaiian music nonetheless survived. In the mid-1850s Hawaiian nationals began to attach political significance to chant and hula as symbols of Hawaiian sovereignty and eventually brought the ceremonies to the palace of King Kalakaua. After the overthrow of Queen Lili'uokalani in 1893 the American-controlled provisional government banned the use of the Hawaiian language in government offices, courts, and schools and thus eliminated the political base for chant and hula. For many Hawaiians the more traditional forms continue as a sacred part of national identity.

Commercial pressures have altered other variations to fit tourist expectations of an island paradise. Although the state economy benefits greatly from the "visitor" industry, these simplified styles trivialize the history of Hawaii and perpetuate the stereotype of the happy-go-lucky Hawaiian. Buck also explores popular, but sometimes unflattering, media portrayals of the islands as seen through the past and present literature on Hawaiian music.

The book has a few drawbacks. Buck insufficiently

relates either the in-depth discussion of scholarly theory or the book's title to her ample coverage of chant and hula. The volume lacks a concluding summary and could use a bibliography. Yet her account of Hawaiian musical development within the larger arena of indigenous cultural change will enlighten many readers. The seven chapters, assembled from primary sources and secondary works, are well supplemented with illustrations, quotations, index, and glossary.

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UNITED STATES

F. ROSS HOLLAND. *Idealists, Scoundrels, and the Lady: An Insider's View of the Statue of Liberty-Ellis Island Project*. Urbana: University of Illinois Press. 1993. Pp. xix, 266. \$39.95.

F. Ross Holland, a former National Park Service official, came out of retirement in the early 1980s to serve as assistant to the president of the Statue of Liberty-Ellis Island Foundation because he believed that private sector involvement in public projects was a good idea. Public-private cooperation was also an early buzzword of the Reagan administration. Like George Bush's "thousand points of light," it had the advantage of sounding high-minded, but what such cooperation meant only became clear to Holland as James Watt's Interior Department, the Park Service, Lee Iacocca of Chrysler, and assorted morons and demi-crooks drawn from the beltway and boardroom did battle over the restoration and interpretation of the Statue of Liberty.

In fact, the Reagan ideologues believed that virtually any business could do society's work better than the public sector could; the rhetoric of public-private cooperation cloaked a festering contempt for government and those who worked in it. The reader will come away from this study marveling, however, that any of these posturing clowns—business bigwigs or federal bureaucrats—contrived to get the statue ready for the patriotic orgy staged at her feet on July 4, 1986.

The 1980s were awash in red-white-and-blue, lump-in-the-throat pageantry, much of it staged for television by David Wolper, who produced the centennial hoopla for the Statue of Liberty (two years earlier, he had filled the home screen with Elvis impersonators during the opening of the 1984 Olympics in Los Angeles). For his gala in New York Harbor, Wolper pulled out all the stops: fireworks, the Reagans and Walter Cronkite in person, an essay contest named for deceased teacher-astronaut Christa McAuliffe, a parade of tall ships, and teary renditions of "America the Beautiful," every element packaged and polished with a bottom-line cynicism that makes one delighted to learn that the event lost money, although you and I and everyone else who made a contribution to the Foundation eventually

picked up the tab. That's how things generally went with the project. The show-biz elements that might turn a profit or enhance the prestige of a board member got the most attention. Fancy brochures. TV commercials. Photo-ops staged around the renovation of especially telegenic parts of the statue. Behind the scenes, life on Liberty Island was one long, vicious catfight to which the Statue and the changing meaning of the immigrant experience became all but irrelevant.

This is not a book for those who hold a high opinion of human nature. Iacocca, president of the several commissions and foundations involved in the restoration (until the Republican Party began to suspect that he was a closet Democrat and presidential hopeful), emerges as a self-important potentate from Detroit, swooping into meetings—or storming out of them—surrounded by a retinue of corporate flacks. And the rest of the business world is not much better: executives connive to build hotels cheek by jowl with national landmarks or to amass enough public-service points on their résumés for new upper-echelon jobs. Architects are pie-in-the-sky artistes with no interest in the practicalities of moving crowds and selling postcards. Political appointees are mean and dumb. Only the long-suffering troops of the Park Service and the immigration historians who drew up the exhibition scenario for the Ellis Island museum emerge from the tale with a shred of honor.

The question of how the public interest is best served in instances in which some detente between public and private sectors seems advisable is an important one, as the recent dispute over awarding concession contracts in national parks to Japanese companies demonstrates. Holland is a nimble guide through the minefield of regulations and practices that govern such relationships. And he makes it clear that ideological purity does not preserve landmarks. But the most fascinating thing about his book is his cast of terrible, pungent characters. I found myself wishing that he had turned his notes into a John Grisham-style novel. The ingredients are all there: greed, inflated egos, blood lust. Of course, the critics would say it was overblown and unrealistic. Nobody could be that awful!

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DONALD WORSTER. *The Wealth of Nature: Environmental History and the Ecological Imagination*. New York: Oxford University Press. 1993. Pp. x, 255. \$25.00.

A generation ago aspiring young professional historians like Donald Worster were warned in graduate schools to be "objective." Values, emotions, and hopes for changing the world were supposed to be carefully suppressed in the interest of "historical truth." Any display of "bias" in a research monograph was cause

for ridicule and scorn in those history seminars of the 1960s.

But we were aware back then that revisionism was the lifeblood of the profession. History was continually rewritten according to the needs and interests of passing generations. Now Worster has drawn the full implications of this axiom, revolted against his training, abandoned what was, in reality, always the myth of objectivity, and boldly placed his heart onto the cover of his book. "The historian," he advises, "should let people know what he cares about and encourage them to care about it too" (p. viii). The senior faculty of Worster's graduate student years must be rolling in their graves, red correction pens poised to strike.

Worster is an environmental historian, and the credo he sets forth in this book is that such people must take on the responsibility of helping reform human conduct toward nature. In this regard he is not very different than, say, women's or African-American historians who frequently have agendas beyond just telling the story. The point is that Worster is not afraid to be an environmentalist as well as a historian of the environment. He believes his craft can be a powerful tool for building ecological understanding and environmental responsibility. It is difficult, in my estimation, to fault such a perspective.

This book is a collection of articles, chapters, and lectures by one of the most capable environmental historians of our time. A few of the pieces are identified as having appeared in journals or books dating back to 1984. It would have been helpful if Worster or his editors had at least indicated when the others were written. Even better would have been prefatory notes telling the reader if and how Worster altered his original work and what he thinks about its importance today. One example of what might have been done appears in chapter 9, "The Kingdom, the Power, and the Water," which contains the seeds of ideas Worster cultivated in his book, *Rivers of Empire: Water, Aridity, and the Growth of the American West* (1985). But we get no indication of either the chronological or ideological relationship between the two works.

Nonetheless, some of these unidentified scholarly objects make for interesting reading. In an essay on John Muir, Worster argues that Protestantism was not, as Lynn White, Jr., has led us to believe, a consistent obstacle to environmentalism but rather, in some minds, a force for reform and evangelical energy on behalf of all God's creation. The chapter that titles the book, unidentified again but presumably a lecture, is intriguing in its contention that from an environmental perspective wealth is actually poverty. This leads Worster to call for a "new Adam Smith" (p. 219) who understands that real economic prosperity can only occur in a healthy environmental context. It is reasoning like this that fulfills the promise of Worster's philosophy that in the hands of

the new environmental historians scholarship and advocacy can go hand in hand.

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B. EDWARD MCCLELLAN. *Schools and the Shaping of Character: Moral Education in America, 1607–Present*. Bloomington, Ind.: ERIC Clearinghouse for Social Studies/Social Science Education and the Social Studies Development Center, Indiana University. 1992. Pp. viii, 135. \$10.00.

Historians of education have long recognized that religious, moral, and civic values played a central role in the emergence of public education in the United States. Nevertheless, this slim volume by B. Edward McClellan identifies and ambitiously attempts to fill a significant gap in the historiography of American education by tracing the shifts in "institutional responsibilities for moral education," the changes in "notions about the nature of moral growth," and the decline "of moral education in the twentieth century school and college" (p. vii).

McClellan covers much ground in few pages. The general trends he identifies seem sound, however, and many are suggestive for more detailed research. Quickly surveying moral education from the colonial period to the present, he notes that British Protestants largely shaped early patterns of moral education that shifted from the predominantly rigid, religious approach of seventeenth-century evangelicals to the less rigid, more secular approach of the late-eighteenth-century gentry.

The opening of American society after the American Revolution brought significant changes. Americans now gave moral education greater urgency, focused their efforts on early childhood, and sharply defined the institutional context for character training: mothers within the family, new Sunday schools, and the emerging system of common schools. Indeed, moral education remained a major task of the public schools until well into the twentieth century. Nonsecularism, forged during the Second Great Awakening, attempted to base American public education on a strong foundation of Christian values while recognizing individual rights to sectarian beliefs.

The most interesting part of McClellan's study traces the decline of moral education in our century, especially after World War II. Seeking harmony, public schools ironically fostered religious divisions as Catholics, Lutherans, and other dissenters created alternative systems. Meanwhile, public schools became more secular as educators emphasized cognitive skills and as more Americans viewed religion and morals as private matters for home and church, a trend furthered by court decisions separating church and state. Within this social context, McClellan describes the fortunes of the three main strategies for

salvaging moral training: preservation of traditional virtues through programs of character education, the fostering of critical moral thinking and situational ethics by progressive educators, and the more traditional grounding of moral values in the religious instruction of parochial schools. He rightly concludes that the fate of these attempts is unclear.

Despite his brevity, emphasis on elementary and secondary schools at the expense of the colleges, focus on the twentieth century, and reliance on published primary sources and standard secondary works, McClellan generally succeeds in his two-fold aim: to provide perspective for the contemporary debate and to stimulate inquiry into the history of moral education.

CARROLL ENGELHARDT
Concordia College

STEPHEN FENDER. *Sea Changes: British Emigration and American Literature*. (Cambridge Studies in American Literature and Culture.) New York: Cambridge University Press. 1992. Pp. xvi, 400.

Stephen Fender has dared to go where no other literary scholar has ventured. As professor at the University of Sussex, widely traveled lecturer on Anglo-American literature, and inveterate "backmigrant" to his native land, this transatlantic and transcontinental Anglo-American transnational has been singularly positioned from "the wrong side of the water" to test "the Anglo-American thesis of the American exception" (p. xv) as shaped by "the rhetoric for and against American emigration by which that discourse was produced, promulgated and challenged" (p. xv).

Surprisingly, heretofore only Dorothy Burton Skårdal has approached this theme in her redoubtably researched, if not quite fully realized, *The Divided Heart: Scandinavian Immigrant Experience through Literary Sources* (1974), of which Fender seems unaware. Whereas Skårdal explored a prodigious if limited body of emigrant literature for what it said about the dilemmas of acculturation and identity formation, Fender, both in his literary method and architectonic design, sets no such bounds. Extending from Amerigo Vespucci to our own day, his commentary on America as "a European idea" is most convincing when he probingly analyzes the "dominant psychology of anglophone emigration to the New World" (p. 10), in particular the Puritan emigrant discourse that he proposes as matrix, microcosm, and paradigm for the rhetoric of national identity, underpinning "the very self-definition of the United States of America" (p. 5).

To sustain so ambitious a scheme beyond the compulsively articulate seventeenth-century Puritan colloquy, Fender faithfully proceeds to examine the rhetoric of the English emigration of the first half of the nineteenth century. Thanks to the pioneering

researches of Charlotte Erickson and Wilbur Shepperson, he is able to draw not only on familiar British travel accounts but also on a host of fugitive letters, sermons, promotional tracts, arcane handbills, and a miscellany of contemporary documents in British and American repositories, all of which, he contends, are as sound literary sources as are the accompanying American canonical texts in illuminating the contrapuntal discourse of emigration.

But then Fender's sensitively honed anglophone disquisition falters precipitously and inexplicably, for it does not extend into the era of massive industrialization after 1870, when more British emigrated to America than in the entire preceding 250 years, an emigration, Maldwyn Jones has argued, that was notable above all for its transience. Even "the Star Spangled Scotchman" of Skibo Castle goes unremarked on, as does Herbert Spencer, Andrew Carnegie's oracle, whose celebrated indictment of Pittsburgh—"Six months residence here would justify suicide"—left no doubt that the anglophone pastoral had been eclipsed. That ever more heterogeneous industrial America Fender invokes only cursorily, obliquely, and iconically. For this book's frontispiece and only illustration (except for the back of the dustjacket copy of Richard Redgrave's "The Emigrant's Last Sight of Home" [1858]) Fender has chosen Alfred Stieglitz's perennially misinterpreted photograph, "The Steerage" (1907), which portrays not arriving but departing immigrants and which the author subtitles enigmatically, "Which way were they travelling?" The photograph, shorn of context in that year of highest "legal" immigration, and perhaps of return migration, ever, as well as of economic recession, is intended to symbolize the perpetual biformity that pervades the whole emigrant experience. But to imply that the notable Puritan return migration during the English Revolution of the early 1640s, in a century where no more at most than one of six emigrants went back, is evidentially exemplary and definitive for all subsequent generations may be metaphorically inspired but is historically misleading.

When Fender, often dazzlingly and ingeniously, carries the discourse of emigration in other directions and watermarks narratives of Indian captivity, MIAs, nostalgia for a lost culture, adolescence, initiation, the West, "lighting out," and other rites of passage, his argument becomes convoluted and the book flounders finally amid allusions to the apocalyptic novels of Don DeLillo and Thomas Pynchon and the Vietnam War movies *The Deer Hunter* (1978) and *Platoon* (1986). The resultant free-flowing topicality, diversity of sources, extended quotations, and digressive suspensions of chronology further undercut Fender's authority. Yet this study remains an important, imaginative, courageous, and original work.

MOSES RISCHIN
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BRIAN J. L. BERRY. *America's Utopian Experiments: Communal Havens from Long-Wave Crises*. (Nelson A. Rockefeller Series in Social Science and Public Policy.) Hanover, N.H.: University Press of New England. 1992. Pp. xviii, 273. Cloth \$40.00, paper \$18.95.

It is a commonplace observation that when you have a hammer in your hand everything starts to look like a nail. And so it is with this first volume in the Nelson A. Rockefeller Series in Social Science and Public Policy, with the hammer being the long-wave-crises theory of the Soviet economist Nikolai D. Kondratiev and the nails being the various communal experiments in America from the seventeenth to the twentieth century. The hammer wielder is Brian J. L. Berry, a geographer.

This book grew out of a research seminar in political economy and a provocative article by Michael Barkun that suggested a link between economic cycles and the growth of communal groups. What has now come forth is a "speculative essay" that probes the relationship between economics, religion, politics, and communal life but is "neither a historian's history based on careful interpretation of original sources, nor a sociologists sociology, based on statistical analysis of community characteristics and trends" (p. xv).

Kondratiev's theory posits that there have been three long waves of economic cycles since the end of the 1780s with both rise and decline phases correlated to the cycles of capitalist economies and produced by changes in technique, wars and revolutions, the assimilation of new countries into the world economy, and fluctuations in gold production. These cycles can be plotted (although they are not regular), and Berry has examined both the statistical data and the social history of American utopianism to see just how they fit into the Kondratiev model. He concludes that they do fit, and that their rise and fall can be traced to the economic cycles.

This is a compelling and challenging thesis, until one begins to look closely at Berry's use of source material. There are eighteen chapters on the history of such groups supplemented by appropriate graphs gleaned from numerous studies, and two summary chapters. Nearly 75 percent of the book consists of summaries of community histories paraphrased (almost copied in several cases) from existing studies, with bibliographic notes sprinkled into the text that suggests scholarship at work. (Oddly enough, there are only endnotes for about half the chapters, with no explanation about the gaps.)

There is neither original research nor much analysis in several chapters, and the text contains errors of fact and proofreading that makes one hope the book does not fall into a reference section. For example, in the section of the Shakers there is considerable emphasis placed on the Panic of 1837 and its impact on Shaker life. Yet there is no acknowledgement that during the 1820s and early 1830s there were high apostasy rates within the Shaker communities. What

the reader receives is only a partial view of Priscilla Brewer's detailed research in *Shaker Communities, Shaker Lives* (1986). Berry's account does little justice to the rise and fall of Shakerdom in America. Here and elsewhere the hammer falls on the unsuspecting nail.

Oneida is treated in the most curious fashion, since there is little exploration of its economy, its financial history, and the reasons that led members to it in the midst of a Kondratiev rise. Only a few members were involved in Millerism prior to their joining, most expressed religious rather than economic motives on joining, and their decline was the result not of a downward cycle but generational conflict and a shift in sexual emphasis. The single-tax colony at Fairhope is reviewed but never analyzed with regard to the land question. The Jewish agricultural settlements (erroneously referred to as part of the "Ad" rather than "Am" Olam movement) are discussed because they came and went, but not as part of some larger cycle that brought them to the United States.

When Berry deals with communal groups in the twentieth century he shifts his focus toward New Deal efforts at resettlement (most family-centered, not communal), Koinonia, and Father Divine projects in the 1930s and 1940s. Outside of these few groups little has been written, and there is a desperate need for a study that looks at cooperative ventures between the wars. Despite the paucity of historical material, the Kondratiev cycle moves on. Berry ends with a discussion of the "Fourth Great Awakening" of the 1960s that produced the "counterculture" and now the "New Age." What we are treated to is some potted history, drawn from J. Gordon Melton's *Encyclopedic Handbook of Cults in America* (1986) but no graphs or charts since no one has been able to produce an accurate picture of such activities.

Berry believes that there has been a "capitalistic-socialistic" dialectic at work since the Labadists established their first group in 1683. Too little credit is granted to religious impulses and actions that arise from spiritual prompting, too little attention is paid to factors other than gross economic ones, and, finally, even in a "speculative essay" of the meta-historical kind, some attention has to be paid to "historian's history," that is, accuracy, attention to detail and nuance, and some respect for the nails that hammers hammer.

ROBERT S. FOGARTY
Antioch College

BARBARA RYAN. *Feminism and the Women's Movement: Dynamics of Change in Social Movement Ideology and Activism*. (Perspectives on Gender.) New York: Routledge. 1992. Pp. xii, 203. Cloth \$49.95, paper \$16.95.

Barbara Ryan's analysis of the U.S. women's rights movement begins with Seneca Falls and ends with its regrouping in response to the Supreme Court's re-

cent approval of restrictive state abortion laws. Her early chapters cover the well-known secondary literature on the movement before 1960 and the "resurgence of feminism" in the 1960s and 1970s, but most of the book describes the women's movement after 1970. A "movement transformation" sociologist, Ryan did her field work, consisting of oral histories with activists from the "second wave of feminism" and participant observation of feminist groups, between 1980 and 1987, when the Equal Rights Amendment (ERA) and its failure was the main concern of liberal feminism.

Ryan's engagement with this period and particular version of feminism has colored her approaches to both the past and present in ways that not only enlighten but also distort her historical narrative. Whereas most historians are leaving the ERA behind as a central focus of women's political history, Ryan sees it as an organizing principle. By not including the articulations of utopian, socialist, and working-class women concerning issues such as pay for housework, class and gender exploitation, and the connection between patriarchy and repressive state policies regarding birth control, her early chapters set the stage for this monolithic view. It is this selectivity that seems to allow Ryan to make Alice Paul the heroine of the first wave of feminism and the unlikely Sonia Johnson the heroine of the second, choices most feminists and historians will find bizarre. The views of the missing Emma Goldman would have illuminated Ryan's narrative in more ways than one.

Ryan conceived of and wrote this book in the mid-1980s, when political paralysis in both academia and the nation at large made it difficult to sort out clearly defined political categories or points of view. Immersed in what many scholars are now describing as a postmodernist, postfeminist era, Ryan's research abandons the study of ideology for political process and supplants mass political constituencies with individual idiosyncracies. Ryan believes that the profound ideological differences between liberals, Left feminists, and cultural essentialists that characterized the bitter debates of the early 1970s were counterproductive, and that the principle difference among feminists and their ideas can be delineated as "small group" and "mass movement," distinctions that most historians will find to be less than helpful. Ryan also wants to show that outside social and historical forces shape group politics as much as do internally generated ideas. But her discussion of this process is relatively unsophisticated and frequently dogged by obscuring social scientese: "What do these examples show? They reveal cycles of intra-movement division and intra-movement cooperation, along with cycles of movement advance and decline—all in interaction with social environmental change" (p. 160).

Ryan did not pursue a statistical profile of groups associated with the second wave of feminism, but instead opted for forty-four interviews with "feminist activists" and observations based on her own mem-

bership in some feminist groups. How the interviewees were selected or what they have in common remains unexplained in Ryan's text, except that many appear to be former stars and leaders of the second wave. Ryan provides a number of intriguing impressionistic quotations; for example, a pro-ERA lobbyist said that she would "rather 'go to jail' than continue to ask for . . . equal rights from 'those small town used car salesmen'" (p. 88). But the possibilities of historical drama evoked in a quotation such as this—of conflicting class background, of a sense of women's entitlement, of the backroom discussions over the ERA that must have gone on in states such as Pennsylvania and Illinois—are never explored. Left and lesbian women who developed what Ryan characterizes as "small-group" feminism and who raised some of the most difficult questions about women's class, racial, and sexual identity in the early women's liberation movement drop out of Ryan's analysis because the groups they formed disappeared in the mid-1970s and were never transformed into national constituencies such as the National Organization for Women. But the women did not disappear, and the history of their organizing at the grass-roots levels of most of the nation's rape-crisis centers, battered women's shelters, women's health collectives and bookstores, lesbian support groups, and so on, is omitted, thus failing to bolster Ryan's assumption that a "feminist subculture" somehow transformed most Americans' thinking about women and their roles in the years between 1975 and 1990.

Ryan is at her best in the final chapters, where she reviews the literature on current political differences in the women's movement and provides a useful and insightful summary of the structural contradictions in the United States that will continue to make women's rights—and opposition to them—part of the political fabric.

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MARC SIMMONS. *The Last Conquistador: Juan de Oñate and the Settling of the Far Southwest*. (Oklahoma Western Biographies, number 2.) Norman: University of Oklahoma Press. 1991. Pp. xvi, 208. Cloth \$24.95, paper \$11.95.

The name given by sixteenth-century Spaniards to the land Juan de Oñate set out to conquer, Nuevo México, gives some idea of their unrealistic expectations regarding the present-day United States Southwest. As Marc Simmons tells his story, Oñate is the tragic hero of the New Mexico story, victim both of false expectations and his own pride. His biography is aptly titled, for it fell to Oñate to conquer the myth that golden cities yet awaited discovery; his was the last bold and reckless drive into the North American unknown. The crown's disappointment at having to assume control of the colony within ten years of its

founding cost Oñate his fortune, his family honor, and his standing within colonial society, which he spent the last fifteen years of his life recovering.

Simmons's book is not a well-rounded biography. Time, war, and a lifetime spent mostly in a frontier setting conspired to deny us the source material for examining much of Oñate's life before he decided to undertake the "pacification" of New Mexico. Simmons extracts the most out of the available documentation, however. The author attaches much importance to the family's Basque background and focuses on Juan's father, Cristóbal, who rose from minor bureaucrat to wealth and power as a co-founder of the rich silver-mining center of Zacatecas. His national background and immediate family history, Simmons argues, gave Juan a sense of mission and destiny that made him an ideal candidate to carry out the New Mexico project.

The bulk of the book is taken up with a detailed examination of the venture. Copious government records, a sympathetic epic by one of the participants, and a variety of legal proceedings and correspondence, most published in translation, allow Simmons to delve into motivations and assess where the enterprise went wrong. The portrait that emerges of Oñate is not flattering. He refused to admit the reality of the situation, became increasingly authoritarian and vindictive toward both Indians and colonists, and was desperate to find some saving grace.

Some unwarranted overgeneralizations and unsympathetic descriptions of the Indians will prove jarring to a sensitive reader. There is, however, much to commend in this volume. Written in a colorful and vivid narrative style, it holds the reader's attention while providing a number of opportunities for comparing the Spanish and English initial colonial experiences.

A decade before the English established their first permanent North American colony at Jamestown and the French founded theirs at Quebec, Oñate led approximately five hundred men, women, and children—Spaniards, Indians, and mestizos—on the last privately funded Spanish colonization venture in North America. Yet he, like Pedro Menéndez de Avilés, founder of St. Augustine, rarely receives more than a passing mention in U.S. history survey texts. This accessible history of the Spanish occupation of New Mexico as told through the life of the expedition's leader may prove a useful way to introduce students to the early Hispanic experience in the present-day United States.

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GERALD R. McDERMOTT. *One Holy and Happy Society: The Public Theology of Jonathan Edwards*. University Park: Pennsylvania State University Press. 1992. Pp. xii, 203. \$29.95.

That Jonathan Edwards was one of the great intellectual virtuosi of the eighteenth century seems to have settled in as an axiom of early American history. What is more difficult to assess is how much real influence he exerted in his time, since his public life has usually been defined only in terms of his involvement in the Great Awakening and his notorious deposition from his Northampton church in 1750. That there was an Edwards who spoke and thought about public life apart from revivals has received only limited attention, and with remarkably contradictory results.

Gerald R. McDermott's short, thoughtful, and eminently well-balanced study of Edwards's "public theology" fills this gap quite admirably, and even where critics are liable to question a conclusion here and there, this book sets out the right questions and the right sources for Edwards's "public theology" in a logical and comprehensive pattern. Public theology, as McDermott defines it, means Edwards's "understanding of civil community and the Christian's responsibility to it" (p. 5). This leads him to an analysis of the salient elements of such an understanding, as they can be found imbedded in Edwards's sermons and writings: the national covenant (the community's present relation to God), the millennium (its future relation to God), citizenship (the community's internal relations to itself), and magistracy (the community and its rulers). In surveying the vast amount of Edwards's published and unpublished texts, McDermott concludes that while Edwards shared many of the conventional New England assumptions about New England's special covenant with God, he used this as a platform for criticism, condemnation, and predictions of impending punishment rather than (in the manner of Sacvan Bercovitch) for covert expressions of tribal optimism.

Similarly, Edwards's millennialism was "dominated by an unyielding concentration on the coming global community" (p. 41) rather than concentration on celebrating a peculiar role for New England or America. McDermott isolates Edwards's famous comments in *Some Thoughts Concerning the Revival* (1742) to show how "the great work" that Edwards believed would originate in America was only a preliminary premillennial awakening that would pick up gradual speed over 250 more years of preparation (the millennium itself would be international in scope, with its headquarters in "Canaan"). Until then, McDermott reads Edwards as promoting neither an ultra-separatist society of regenerate come-outers, nor a feudal society of religious mediocrities. Edwards was fundamentally indifferent to politics per se, but his peculiar ontology of being drove him to look for the structural interdependence of all beings with each other and the consequent exercise of disinterested benevolence. For that reason, although Edwards was no secular egalitarian, he held magistrates to a clearly defined set of responsibilities and functions, which included "friendly but distanced support to true religion" (p.

134). It is in this interdependence that McDermott believes he has located a "subversive" Edwards: a theologian whose commitment to personal regeneration, and to a leveling skepticism about deference, hierarchy, and elitism apart from grace, may have had its own mite to contribute to the revolutionary mentality of the 1770s.

This is, of course, quite a stretch of intellectual territory to cover, and some special pleading does creep in from time to time, especially when McDermott strains to cast Edwards's shadow forward to anticipate the revolution or women's rights or the struggle of Native Americans. But in this, he only partakes of a mysterious preoccupation of Edwardsean interpreters (from Perry Miller to Sang Lee and Robert Jenson) with Edwards's "modernity" or "present force." Happily, McDermott has enough healthy skepticism of his own about "modernizing" Edwards (a skepticism I suspect which owes much to McDermott's dissertation adviser, T. Dwight Bozeman) to keep that temptation from getting out of hand. At those moments, however, we lose a clear sight of Edwards in his own century; and what is more, while it is certainly a relief to spring Edwards loose from the Connecticut Valley, McDermott's desire to give him an international millennial perspective overshoots the critical transatlantic perspective that may be the best context for understanding Edwards's millennialism. What does clearly emerge from McDermott's gracefully written portrait of Edwards is a figure of demanding, almost frightening rectitude, a voice fashioned around an eighteenth-century theological vocabulary but a voice also capable of rising above that century to describe not modernity but a city not made with hands.

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RICHARD GODBEER. *The Devil's Dominion: Magic and Religion in Early New England*. New York: Cambridge University Press. 1992. Pp. x, 253. \$24.95.

Since the publication of Keith Thomas's *Religion and the Decline of Magic* (1971), historians of early America have tested on their own terrain his many insights into the relationship between religion and magic in England. In the process, they have forged ever stronger links between popular religious beliefs among early modern Europeans and their colonial counterparts. Richard Godbeer's study is the latest effort to sort out what ordinary Euro-Americans believed and practiced, and to evaluate the multiple meanings of magic in a setting where a reformed Christianity dominated the cultural and political landscape.

His focus is New England and his time frame the seventeenth century. He argues that English settlers brought with them a deeply embedded magical tradition along with their officially sanctioned Christian-

ity, that Puritan authorities roundly condemned magic as the Devil's work but failed to eliminate its practice, and that popular faith in the efficacy of magic was the source of considerable community tension and overt conflict. Countermagic was also pervasive, as was ministers' insistence that divination was as blasphemous as attempts at supernatural harm. Lines between Puritanism and magic were not stable, however; on the contrary, affinities between them, Godbeer says, encouraged even church members to resort to one or the other. Overlap and ambivalence, not neatly compartmentalized belief systems, characterized New England's popular and elite cultures.

The final two chapters deal with New England's witchcraft trials. Malefic as well as harmless magic was practiced, Godbeer avers, by at least some women. But the focus of these chapters is less witches and their accusers than problems created for the courts by two distinct definitions of witchcraft—one shared by the religious authorities, the other by the larger population—and internal tensions created in the region by a series of disasters, most notably attacks by local Native Americans and political, economic, and religious interference from England. Because witnesses rarely testified to witches' alliances with the Devil, which was legally required for conviction, a high acquittal rate in the post-1660 decades fostered increasing popular resort to countermagic to ward off what was experienced as personal and community "invasions." The Salem outbreak of 1692, then, while having multiple causes, is best explained by an official and popular predisposition to blame their many troubles on external forces.

Much of this argument is not new. Colonial historians have documented the ubiquity and durability of magical beliefs and practices, the clergy's distinctive opposition to magic and their insistence that witchcraft be understood as Satan worship, the series of events that led eastern Massachusetts to search frantically for supernatural explanations as the century drew to a close, and the way individual accusations of witchcraft served to thrust out the "witch" within. Not everyone has come to the same conclusions, but these several interpretations have been firmly established and the core issues of debate identified.

What Godbeer offers is more extensive evidence, particularly for the persistence of divination, healing arts, and astrological discourse. He also offers a kind of synthesis of the last quarter-century's scholarship on religion and magic, although his privileging of cultural over social formations renders the book only partially useful as an overview of current controversies. In skirting the issues of why only some practitioners of magic were tried as witches and why for condemned witches evidence of malefic practices is either lacking or extremely tenuous, Godbeer fails to establish convincing links between magic and the witchcraft trials. His book deserves attention mostly for its sensitive and detailed rendering of the compli-

cated religious world view that characterized "Puritan" New England.

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DAVID E. NARRETT. *Inheritance and Family Life in Colonial New York City*. Ithaca: Cornell University Press or New York State Historical Association, Cooperstown, N.Y. 1992. Pp. xiv, 248. \$41.50.

David E. Narrett's book is a beautifully written and comprehensive examination of inheritance practices in colonial New York City. It covers standard topics, such as the treatment of widows, the distribution of property to sons and daughters, and the naming of executors, but it also discusses to good effect matters not always included in existing social histories of inheritance, such as the social backgrounds and training of the notaries and lawyers who helped prepare marriage settlements and wills. Narrett focuses on families of Dutch descent, but he strengthens his findings by comparing the practices of the Dutch with English testators and other ethnics, and by including a sample of rural testators.

The book's chief interest lies in comparisons with the existing literature on inheritance among Anglo-Americans. Although Narrett finds that there is some Anglicization of inheritance practices among the Dutch in the eighteenth century, the slow accumulation of detail reveals a pattern of inheritance quite different in its essentials from practices among Anglo-Americans despite the reception of English common law in New York after 1691. (It is a tribute to Narrett's prose that he can discuss with grace and economy the tumultuous era of political transition after the English capture of New Amsterdam in 1664 and the subsequent interaction of Dutch and English law in New York.) To give only two important examples, the Dutch, following longstanding customs linking marriage and community property, gave priority to spouses rather than to children. This is revealed most dramatically in the willingness of men who had children to give all their property—including life-estates in all land—to their wives. Whereas some men became less generous in the course of the eighteenth century, a majority were still adopting this practice in the 1750s. Such men postponed their children's inheritance, trusted their wives to manage family estates and businesses, and allowed them to make decisions about the eventual distribution of personal estates. This is a family cosmos quite unlike that of most contemporary Anglo-Americans. Similarly, the Dutch were radically egalitarian with respect to children, almost always opting to give equal shares to sons and daughters. In contrast, Englishmen, even when they treated their sons equally, tended to short-change daughters. These distinctive Dutch practices were more marked among urbanites, as we might

expect, but by no means disappear among Dutch yeomen.

The significance of these and other findings is great. It suggests that the Dutch system of familial and household authority is not patriarchal. These inheritance practices are part of a larger complex of law and practice that enabled Dutch women, including she-merchants, to undertake public economic activities in large numbers on both sides of the Atlantic. They are indicative of a cultural and legal system that, although not egalitarian with respect to gender, permitted a more autonomous civil status for women than in England or the British colonies. Although the empirical verdict on women's public roles in early America, especially in urban America, is still not in, Narrett's book adds to an accumulation of evidence suggesting that Dutch culture appeared to sanction women's public presence in a way that Anglo-American culture did not. Thus, this study, along with others of its kind, underscores the diversity of law and custom within the sometimes nominally British mainland colonies, and it should remind us not to universalize the experience of the English in America.

Despite the excellent prose, key data are occasionally not presented clearly. In some passages, for example, we do not know whether widows have been given ownership of land or life-estates, although the unwary reader is likely to assume that the former is the case. Similarly, although the book contains an interesting discussion of lifetime gifts to children, we are not always certain whether Narrett's finding of equal treatment is based on wills alone or on wills in combination with lifetime transfers. Narrett is also perhaps too understated about his conclusions. His is a book rich with implications, but he opts for gentle interpretive hints rather than systematic elaboration. Still, the book is essential reading for those interested in colonial inheritance practices or the Dutch social experience in British America. Social historians interested in colonial port cities and in ethnicity will also find it instructive.

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SAMUEL H. BEER. *To Make a Nation: The Rediscovery of American Federalism*. Cambridge: Belknap Press of Harvard University Press. 1993. Pp. xxi, 474. \$29.95.

In this well-polished polemic, Samuel H. Beer deploys, in defense of the constitutional doctrines espoused by Alexander Hamilton, Abraham Lincoln, Herbert Croly, and the advocates of the New Deal and the Great Society, the fruits of six decades dedicated to the study of British and American political theory and practice. Beer aims, in particular, at refuting a "conclusion," which he attributes to Martin Diamond, "that the federalism of the framers, far from having a coherent rationale, combined two incompatible forms, one state-centered, the other

nation-centered." To this end, Beer sets off in "quest for a usable past" that will nonetheless be "faithful to that past as it actually was" (pp. viii, xi). If he succeeds to an impressive extent in his quest, it is a tribute to his great learning and intelligence.

In part 1, Beer lays the groundwork for his argument, juxtaposing the case made by Thomas Aquinas, Richard Hooker, and James I for the direct rule of the wise and the holy with the arguments advanced by John Milton and James Harrington on behalf of indirect, popular rule in an extended, federal republic. In part 2, he compares the thinking of Edmund Burke and William Blackstone concerning virtual representation and the supremacy of Parliament with the arguments advanced by the colonists on behalf of actual representation and the sovereignty of the people. And in part 3, he contrasts the confederate republicanism of Montesquieu with the compound republicanism of James Madison, arguing in the process for the ultimate superiority of James Wilson's understanding of the location of sovereignty and the nature of the American union.

Beer's writing is forceful and evocative, his learning is impressive, his treatment of the various authors whom he considers is usually judicious, and his argument throughout is provocative and stimulating. No one who peruses this thick tome will fail to learn much of value. But the book has defects as well.

Beer sometimes bends the thinking of the writers he discusses to fit his case. There is, for example, a much greater gap between Milton and Harrington than he concedes. It is perfectly appropriate to term the author of *Areopagitica* a proponent of "government by discussion," but it is impossible to square such a description of the author of *Oceana* with that imaginary republic's prohibition of public debate on penalty of death. Similarly, Blackstone was much closer in his thinking to John Locke than Beer is inclined to admit. Certainly, no one will recognize in his Blackstone the writer whose commentaries on law the colonists considered a textbook of revolution.

Beer also devotes too little attention to the proponents of states' rights. He touches on the Anti-Federalists but gives them short shrift. Nowhere does he provide an extended discussion of the thinking underlying the Virginia and Kentucky Resolutions. Nowhere does he lay out in detail the arguments advanced by John C. Calhoun and his many admirers and allies. His argument would have been more persuasive if he had taken the time and trouble to present in greater detail the case made by those who, in varying degrees, sought to limit the size and scope of the federal government. Had he immersed himself in this literature, he might have ended up being more appreciative of the case made by Diamond for the view that the framers of the Constitution managed to strike a balance between the rival claims of nation and state.

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WILLIAM LEE MILLER. *The Business of May Next: James Madison and the Founding*. Charlottesville: University Press of Virginia. 1992. Pp. xii, 296. \$24.95.

William Lee Miller's engagingly written and thought-provoking book is an intriguing blend of an older, triumphal Federalist and a newer, more somber republican historiographical tradition. Although it stands very well on its own as a self-contained unit, this work is actually intended to be the first of four studies of the "moral foundations of the American Republic" (p. xi), the larger contours of which Miller does not define further. Part biography, part political narrative, and part philosophical meditation, the present work addresses this central theme by dealing with James Madison's role in effecting the constitutional revolution of 1787, which replaced the league of sovereign states created by the Articles of Confederation with the federal system of divided sovereignty that has made the United States the world's oldest surviving republic.

Ably summarizing current received scholarly wisdom, Miller argues that Madison's most lasting claim to historical greatness rests on his critically important contribution as a theorist and as a statesman to the creation of an enduring republican political order in the United States. Confronted during the 1780s by a deep crisis of republicanism marked by rapidly weakening central authority, resurgent localism, declining civic virtue, and widespread agrarian distress, Madison, as Miller shows with great insight, reformulated the basic presuppositions of classical republican thought. Rejecting Montesquieu's dictum that a republic could only flourish in a small political unit, Madison concluded, with the aid of some timely hints from Hume's writings, that a continental republic would in fact provide the best guarantee of republican liberty by bringing into play a multiplicity of opposing interests.

Having explored the intellectual roots of this key insight, Miller then deals at great length with Madison's role in the Philadelphia convention. He carefully shows how this august body rejected some of Madison's more extreme proposals for curtailing the authority of the states, shrewdly notes how the grant of equal state representation in the Senate led Madison to take a more active interest in strengthening executive and judicial authority, and rightly emphasizes the importance of Madison's political skills as a negotiator and compromiser. Miller carries Madison through the ratification struggle and the adoption of the Bill of Rights, stressing once again Madison's enviable ability to combine the powers of an uncommonly fine analytical mind with the skills of an unusually successful practical political leader. Although scholars will find little that is original in all this, Miller deserves credit for synthesizing a generation of scholarship on Madison and republicanism for the benefit of a wider audience.

Despite its virtues, Miller's book is not entirely

successful in placing Madison's achievement in proper historical perspective. Late in life Madison insisted that no one could properly assess his contribution to the Constitution without first understanding the crisis of the 1780s that led up to it, and this is a challenge Miller often fails to meet. Miller is too prone to attribute this crisis to the deficiencies of what he sees as an inherently weak and somewhat ridiculous Continental Congress—this of the body that unified American resistance to British imperialism, sustained a long war against the world's mightiest empire, pursued the diplomatic strategy that paid enormous dividends for the United States in 1783, and established a territorial system that facilitated the subsequent continental expansion of republicanism. A more nuanced view of the breakdown of central authority in the United States and closer attention to what Madison perceived to be majoritarian tyranny on the state level in the 1780s would have enabled Miller to bring into better focus the crisis of republican virtue that inspired Madison to spearhead the constitutional revolution of 1787. For it is clear that Madison was then responding as much to perceived failures in the American national character as to actual failures in American institutions, a point that Miller recognizes to some extent but one whose full complexity he does not capture.

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LYDIA DITTLER SCHULMAN. *Paradise Lost and the Rise of the American Republic*. Boston: Northeastern University Press. 1992. Pp. xiii, 273. \$45.00.

This study of *Paradise Lost* and classical republicanism in eighteenth-century America assesses John Milton's influence on the leaders of the American Revolution and the early republic. Fashionably, Lydia Dittler Schulman "historicizes" the epic by locating it in its seventeenth and eighteenth-century political context. Unfashionably, she takes issue with the New Historicists, feminists, and leftist scholars in general who have condemned Milton as a voice of elitism and male authoritarianism. She concedes that Milton is flawed by current standards of gender relations, but she insists that to reduce his meaning to bourgeois misogyny is to miss the real point.

For Schulman, the real point is Milton's argument that republican liberty, based on an educated, virtuous citizenry, must meet the challenge of controlling narrow self-interest through enlightened reason. At least this was what a peculiarly receptive and sympathetic audience in eighteenth-century America found in Milton's work, and Schulman believes that these readers, not the Romantics and their twentieth-century descendants, read Milton best. Consequently, Schulman is not of "the Devil's party" in her interpretation of Milton's Satan. Rejecting the Romantic view of Satan as a heroic character (a view shared by

Thomas Jefferson), Schulman argues that for Milton and his ideal, non-Jacobin readers, Satan was a selfish, corrupt—and corrupting—demagogue. In her careful exploration of eighteenth-century American readings of *Paradise Lost*, she makes it clear that, while political leaders had trouble agreeing on who among them was best qualified to play Satan, they generally agreed that the role was not flattering. All sides—Whigs and Tories, Federalists and Republicans—"Satan-baited" their opponents" (p. 14), although it seems that from Schulman's perspective only the "Jacobins" were truly right for the part.

How "right" is Schulman's approach to Milton and his epic? Contemporary "Rebel Angels" may be tempted to make an easy, and superficial, response. But, ideology aside, two complications threaten to limit her book's appeal. First, although Schulman resists reading the epic "as a species of political allegory" (p. 12), the poem's meaning remains, for her, largely political—a reading that risks diminishing Milton and his greatest work. Second, Schulman is on much safer ground while trying to place Milton in his own century's political environment than while tracing his influence in eighteenth-century America, where he was only one of several influential republican writers. Whatever Milton's literary stature may have been (compared, say, to Alexander Pope's), all classical republicanism was not Miltonic. Schulman argues that "the widespread borrowing of Milton's poetic imagery and the frequent invocation of his authority on points of republican political theory clearly establish his central place in late eighteenth-century American culture," but she must also admit that "the direct influence of Milton's texts on revolutionary Americans cannot be demonstrated, in a strict sense, by frequent citation alone—and is indeed very difficult to prove definitively" (p. 227, n. 37).

But if Schulman inevitably fails to prove her thesis "definitively," she succeeds admirably in suggesting the complexity of Milton's role in the still lively debate over the soul of the republic. Her book is a strong contribution to that debate.

JOHN CANUP
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EDITH B. GELLES. *Portia: The World of Abigail Adams*. Bloomington: Indiana University Press. 1992. Pp. xviii, 227. \$24.95.

The best thing about this best-of-all-biographies of Abigail Adams is that it is not really a biography at all. It is a collection of independent essays, each of which tells us as much about Adams's gender-determined world as it does about Adams herself. Equally important, Abigail, not John, is at the center of every chapter, making the episodes a series of stories about Abigail Smith Adams rather than Mrs. John Adams. In brief, Edith B. Gelles uses the revolutionary years as the backdrop of this sensitive study, and the

political events as the drama in which the players act out well-defined roles.

As an eighteenth-century wife and mother, Abigail was expected to subordinate her own needs to those of her husband and children. And in this case, virtue demanded that she suppress her own private inclinations for the public aspirations of a nation. Abigail did what was expected of her, but she did so reluctantly and ambivalently. She aspired to patriotism, but grieved that she was so frequently parted from the man she loved. Gelles effectively demonstrates that in her heart of hearts Abigail resented the idea that John and her country called on her to make this sacrifice.

Constructed as it is, this book has the potential of being a collection of disjointed parts. Gelles does not allow this to happen because most of the chapters focus on a particular relationship, giving cohesion to the whole: Abigail and her husband, Abigail and her sisters, Abigail and her sons, Abigail and her daughter, Abigail and Thomas Jefferson.

One of the most interesting chapters is the one about Abigail and James Lovell. Lovell, a friend of John and congressional delegate from Massachusetts, engaged in a correspondence with Abigail. This exchange conformed to eighteenth-century gender role expectations, but, at the same time, it served a variety of purposes. Most simplistically, it mitigated Abigail's loneliness. On another level it brought her up-to-date news about John. Yet it was also a private theater in which the female character could express herself in ways that were otherwise impermissible. It is not too much to say that the epistolary confrontation between Adams and Lovell was a struggle for power, in much the same way that the Adams-Jefferson dialogue was. Equality between a man and a woman was an unrealistic prospect in real life, but letter writing bordered on the fictional, similar in many ways to *Pamela* and *Clarissa*. We will never know what John Lovell and Abigail Adams might have said to each other face to face, but Gelles presents the interesting possibility that letter writing offered opportunities for parity that personal contact denied.

The fact that Gelles takes a firm position on certain issues that may still be open to debate, or that she assumes the veracity of certain historical truths about women that may eventually be revised, is of minor importance given the significance of her achievement. One might argue, contrary to Gelles's assertion, that Abigail did indeed think "inventively about politics" (p. 13). She extracted sentences from English writers and adapted them to her own purpose. She connected the arbitrary power of monarchs to the tyrannical power of husbands, which was creative in the American context even if European feminists had developed this idea a century earlier. One could even argue that women were "driven to political action as were men" (p. 91), but simply responded differently to the same stimuli in deference to their prescribed social roles.

Further investigation may indicate that a number

of eighteenth-century women were as educated as Abigail and her daughter, making them less extraordinary (p. 156) than we thought, and other research will probably show that it was not unusual for women to be examined by physicians before the mid-nineteenth century (p. 162). But these points are minor given the scope of Gelles's work. Her story of relationships, networks, and power in the context of Abigail's eighteenth-century world is truly a superb accomplishment.

ELAINE FORMAN CRANE
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WINIFRED BARR ROTHENBERG. *From Market-Places to Market Economy: The Transformation of Rural Massachusetts, 1750-1850*. Chicago: University of Chicago Press. 1992. Pp. xiv, 275. \$37.50.

This work is Winifred Barr Rothenberg's spirited contribution to the ongoing debate about how thoroughly and willingly early Americans were involved in a market economy. Drawing together and refining work that she has previously published in a series of widely read articles, Rothenberg presents a direct challenge to the "moral economy model" of the early American economy, at least as applied to New England, the study of which region has produced most of the contentious literature. Others have argued, Rothenberg states, that New England farmers resisted market involvement: that they clung to collective, community-oriented economic decision making rather than choosing individually what to plant and market, and that within their community they bartered goods and labor based on "use value" rather than buying and selling at a market price. Rothenberg argues that this notion of a "moral economy," one that puts community welfare above individual advancement, derives from current interest in Marxism and cultural anthropology and does not describe accurately or meaningfully (as it cannot be verified) the world of New England farmers. She posits that a market economy evolved in Massachusetts only when social and political restraints on individual economic decision making were stripped away. An economy based on market places (towns and fairs) was transformed into a self-regulating market economy that was more than the sum of its parts (individual traders and specific marketplaces). This great transformation had begun by the 1750s (the "proto-market process") and, between 1785 and 1800, decisively reshaped Massachusetts's agriculture.

Rothenberg's conclusions are based on analysis of Massachusetts town tax valuations and probate records and on the most comprehensive use to date of farm account books. These sources allow her to measure such things as the convergence over time of local crop prices and crop prices in regional (urban) markets, the parallel convergence of wage rates, the distance farmers were willing to travel to market their

crops, the shift of capital from land into more liquid holdings (cash, bonds, and the like), the changing mix of day and contract labor, and productivity growth in agriculture. From such measurements, Rothenberg argues that the market economy (and with it, the "culture of capitalism") increasingly penetrated rural society, that the agricultural economy became more productive over time, and that the shift of resources out of agriculture and into industry was a major factor in the sustained development of Massachusetts's economy as a whole.

However historians assess the arguments in the "moral economy" debate, this is a study with which they must come to terms. This stated, it remains doubtful whether Rothenberg's work, or any other, will settle the "moral economy" debate. Those who argue that New England farmers held the market economy at bay focus on the cultural "meaning" of individual behavior; she, in contrast, studies the measurable, aggregate consequences of that behavior.

And while Rothenberg astutely notes that working from a frame of a moral economy makes possible the creation of a before-and-after story of encroaching capitalism and social disintegration for virtually any period of American history, she, too, begins her analysis at a point (1750) when many others might speculate that the moral economy of rural New England was quite dead (if, in fact, it ever existed). Also troublesome is the absence of consideration of women and the farm household (what a wonderful opportunity to put her theoretical skills to work); by the crude analysis of Shays's Rebellion (surely, traditional sources, which give evidence of the participants' own understanding of their actions, are of importance here); and by Rothenberg's all-too-brief speculation that the driving force behind agricultural change was a long-term change in New England's climate. None of these problems, however, diminish the significance of this provocative study.

PAUL G. E. CLEMENS
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ALBERT H. TILLSON, JR. *Gentry and Common Folk: Political Culture on the Virginia Frontier, 1740-1789*. Lexington: University Press of Kentucky. 1991. Pp. 228. \$30.00.

R. EUGENE HARPER. *The Transformation of Western Pennsylvania, 1770-1800*. Pittsburgh, Pa.: University of Pittsburgh Press. 1991. Pp. xx, 273. \$39.95.

For some time now, historians have fruitfully explored early American communities in their local settings. Many have focused on New England towns and Chesapeake counties while others have examined colonial cities. Albert H. Tillson, Jr.'s study of Virginia's southwest in the mid-eighteenth century and R. Eugene Harper's examination of southwestern Pennsylvania in the late eighteenth century expand the

reach of local history to the late-colonial frontier. By doing so, these authors confirm the continued value of community and regional studies.

Tillson's work is more a political history than a social history. Yet the social character of Virginia's upper valley is essential to his story. First, he finds that the political leaders of the upper valley were very much proponents of the elitist political culture of eastern Virginia, and that they expected their orders to be obeyed with deference. Tillson also finds, however, that the upper valley's lesser sort gave deference grudgingly, and in military matters they frequently defied their leaders. This resistance, Tillson argues, came about because the lesser folk, scattered in small neighborhoods and pursuing a small-scale agriculture, had interests at odds with those of their regionally oriented leaders. Although the political elite retained control, Tillson concludes that this contest of interests, especially during the American Revolution, led to a less hierarchical political culture on Virginia's southwest frontier by 1789.

Tillson makes excellent use of the surviving correspondence of the upper valley's political leaders. He makes an especially convincing case that the leaders saw themselves as part of the colony's elite and shared similar political values. He even conducts an intriguing analysis of one of the area's early histories. Although this examination jars with that used elsewhere, it adds to the strength of his conclusion. His examination of the upper valley's frontier society, however, is less fully developed. Just how long did the region's economy remain noncommercial? Did an open access to property persist between 1740 and 1789? Answers to these questions, as well as ones about the importance of the upper valley's ethnic and religious diversity, could have bolstered his argument that conditions on Virginia's southwestern frontier promoted a democratic political culture.

Harper's examination of southwestern Pennsylvania is a close quantitative study of the region's class structure. The Monongahela River basin attracted settlers until the early 1790s, when the overall settlement of southwest Pennsylvania was completed. Harper finds that within the short span of one generation the distribution of property there mirrored that of the longer settled areas of the east. For example, across the period, the proportion of landowners had fallen to less than 60 percent, and wealth held by the rich had increased. Furthermore, a wealthy elite controlled the region's political affairs. A growing number of towns helped speed this process by attracting propertyless as well as nonfarming residents. Urbanization, coupled with early industrial development, transformed the economy of the region by the 1790s. Artisans, laborers, tenants, and the poor were then as familiar faces on the land as was the freehold farmer. From this evidence of economic maturity, and social inequality, Harper concludes that

the Monongahela region ceased being a frontier in less than thirty years.

Harper's use of his principal source, property tax lists, is careful and balanced. He is well aware of their shortcomings and his control of their biases is credible. Furthermore, his use of statistics is judicious, and he discusses statistical averages in terms of actual individual experiences. His study is a model of how statistics should be used. Yet the use of tax lists necessarily narrows the focus of his inquiry, and his statistical approach is more descriptive than explanatory. He only hints at why this region matured so quickly and leaves unquestioned the assumption that frontiers were initially areas of equal opportunity. The tight focus on class slights other cultural aspects of frontier life that may also help define what a frontier is.

Although Tillson and Harper reach very different conclusions about the frontier societies they examine, their studies do provide a surprisingly similar insight. From the start, frontier areas were never places apart, but instead were closely tied economically, socially, and politically to coastal societies. The backcountry may have developed differently, but never in isolation. These two studies remind historians not to generalize too easily about frontier life.

KEVIN P. KELLY

Colonial Williamsburg Foundation

JUNE NAMIAS. *White Captives: Gender and Ethnicity on the American Frontier*. Chapel Hill. University of North Carolina Press. 1993. Pp. xix, 378. Cloth \$39.95, paper \$16.95.

This is an insightful and provocative study. June Namias uses a feminist perspective to explore gender and ethnicity in the captivity accounts of Anglo-Americans. She suggests that captivity stories "helped the Euro-American culture struggle through questions of cultural and gender identity during periods of extreme change and uncertainty" (p. 11).

Namias analyzes captivity literature for the period 1607 to 1870, working her way from New England to the Dakotas. She also examines portrayals of captives in dime and other novels, in paintings, and in plays. From her extensive research, Namias identifies three female models: the "Survivor" during the colonial period; the "Amazon" during the revolutionary era and early republic; and the "Frail Flower" during what Namias terms the "era of expansion," 1820 to 1870. She persuasively argues that each type reflected the era's prevailing social beliefs regarding women.

Namias also considers male captives, categorizing them as "Heroes" and "white Indians." Evidently, there was no room for frail flowers here, for, according to Namias, Native American captors eliminated those white male captives who could not cope. Nor does Namias see as close a tie between male archetypes and society's gender expectations.

Next, Namias examines sexual relationships between captives and captors, stressing the differences between female and male captives. She notes that although reports of rape and sexual abuse rarely occurred during the colonial period, they swelled during the nineteenth century and carried a cautionary message, that "Indian and white unions are bad business" (p. 97).

Namias then turns to an exposition of three women's captivity accounts, which consumes over half the book. All three women were well-known and well-published or well-represented captives: Jane McCrea, Mary Jemison, and Sarah Wakefield. According to Namias, none neatly fit either her typologies or time periods, but they still provide illuminating case studies.

Generally, Namias supplies a scrupulously researched and carefully appraised perspective. Still, the resulting book is not without problems. For one, the title is misleading. In truth, "white captives" and "gender" means women captives and the female gender. Male captives and the male gender receive short shrift, providing twenty-five sources compared to forty women's accounts. Men also figure little in the overall analysis, illuminate little about societal expectations of their gender, and rate no case studies.

In the book's conclusion, Namias argues that women and children deserve more notice because they provided the "focus of attention" (p. 264) in captivity accounts, an assertion that cries for proof such as textual analysis, sales figures, and other publication statistics. Namias also believes that the assumed emphasis on women in captivity tales "recognized the centrality of women and children in Anglo-American life on the frontier" (p. 264), again an unproven assumption. If women were indeed central in captivity accounts, it may have been simply because female captivities often involved sexual titillation, a subject that sold, then as now.

Besides delving deeper, it would be helpful if Namias's study went farther. Stopping in the Dakotas in the early 1860s leaves an important segment of westward migration, white-Indian conflict, and captivity unexplored. Representations of Indian captors in circuses, stage plays, and Wild West shows after the Civil War also remain unanalyzed.

Finally, Namias's conceptualization raises a philosophical issue. Namias has chosen to study white captives from the viewpoint of Anglo society. When she writes of culture, society, and gender constructions, she clearly means Anglo culture, society, and constructions. This is fair; it is her book and she can delineate her approach. But, in this time of cross-cultural perspectives, other scholars must take up the topic and enlarge our understanding of it. What, for example, did Native Americans think of the Anglos they captured? And did captivity create any bridges between the two groups? Unpublished captivity documents, which are not part of Namias's study, contain some insights; Indian sources may offer others.

Moreover, what of the Anglo practice of taking

Native Americans captive? How did Indian women and men fare in captivity; what did Indians think about the captivity of such leaders as Chief Black Hawk, who during the 1830s was first jailed in St. Louis, then toured around the United States on public display; and how did Indian captivity stories help native cultures confront questions of cultural and gender identity in a time of change and uncertainty?

What Namias provides, then, is a useful analysis and an impetus for other scholars. She has revived interest in an almost moribund topic, identified some valuable models, and pointed the direction for further research and study.

GLEND A RILEY
Ball State University

WILLIAM B. SKELTON. *An American Profession of Arms: The Army Officer Corps, 1784–1861*. (Modern War Studies.) Lawrence: University Press of Kansas. 1992. Pp. xvii, 481. \$45.00.

Historians have traditionally looked to the late nineteenth century for the emergence of a professional army in the United States. William B. Skelton's examination of the character, composition, and outlook of the American officer corps in the years before the Civil War suggests a very different view. Influenced by the military traditions of Europe, nationalism and Manifest Destiny, the mounting importance of technology in military affairs, and a military culture rooted in a West Point education and nourished through lengthy careers, a small corps of professional officers dominated the U.S. Army in the decades after 1815.

Little in the army's history before the War of 1812 predicted the professionalization of the officer corps during the antebellum period. The republic's founders linked professional soldiers with tyranny and looked to courage more than expertise to ensure national security. Thus, political credentials frequently outweighed military experience in the awarding of commissions, retarding both a professional ethos and a national orientation among military leaders. Frequent reductions in force, combined with the amateur outlook of many officers and low pay made turnover high and careers short. Duelling, commonplace in the early republic, underscored a general uncertainty about the conduct appropriate to officers in a republic. Only in the technically oriented engineering and artillery branches did professional standards take hold before John C. Calhoun remodeled the army during the administration of James Monroe.

Skelton attributes the growing professionalism of America's military leadership to a number of factors. The stable size of the army after 1821 allowed for both long and predictable careers. The near domination of West Point graduates at all but the highest levels of the command structure infused educational standards, a broadly accepted code of conduct, an

apolitical approach to military affairs, and a national vision of service. Military professionalism also developed in a climate receptive to occupational specialization and bureaucratic institutions. The army's principal mission also shaped its commitment to professionalism. Although the history of the nineteenth-century army—excluding the Civil War years—is usually associated with the containment and conquest of Native Americans, its principal mission was the defense of the republic from external attack. The ability to lead an expanded army of citizen-soldiers against an invading European force guided the education, training, and aspirations of the army's leaders from the 1820s on. Indian warfare received virtually no attention at West Point. Moreover, while many officers spent their careers fighting Indians, they considered that duty distasteful and a distraction from the army's legitimate mission.

The officer corps attracted young men of social and political—if not economic—prominence; men with ties sufficient to secure appointments to West Point. Motivated by romantic notions of military service, family traditions of public service, and happenstance, these men brought to the army the social values and prejudices of the republic's middle and upper middle classes. Nevertheless, while nativism manifested itself in the contempt officers had for the immigrant-filled enlisted ranks, army leaders stood above the sectional conflict. With discussions of slavery banned at West Point and political activity limited largely to securing promotions and branch lobbying, most officers served the nation above the political fray. Even when secession made decisions unavoidable, family loyalties rather than politics seem to have motivated those resigning their commissions to join the Confederacy.

Skelton has successfully recast our understanding of the officer corps in antebellum America. Broadly conceived, thoroughly researched, and well written, his book gives depth and insight into the nineteenth-century effort to build a military establishment.

LAWRENCE DELBERT CRESS
University of Tulsa

CARL A. BRASSEAU. *Acadian to Cajun: Transformation of a People, 1803–1877*. Jackson: University Press of Mississippi. 1992. Pp. xiv, 252. \$16.95.

During the nineteenth century, Louisiana's Acadian community experienced important demographic, cultural, political, and social changes. In this study, Carl A. Brasseaux cogently explores this metamorphosis. Because newspapers, personal papers, diaries, and other traditional primary sources are scarce, Brasseaux uses census records, church and court documents, and election returns to chronicle the history of the Acadians during this crucial period that includes the secession crisis, the Civil War, and Reconstruction.

At the core of this study is the impact of economic

and social change on the Acadians. Brasseaux maintains that the Acadians split into two groups. One was a well-to-do contingent that emulated and commonly merged with the more prosperous Creoles and Anglo-Americans. This group frequently intermarried with the upper classes and lost much of its cultural distinction. A second, less-successful, group remained separate. Brasseaux argues convincingly that this group also absorbed unfortunate members of the Creole community. The product of this merger was the people whom Americans now call Cajuns.

Brasseaux, however, is careful to note that the Acadians of southwestern Louisiana did not simply divide into two major camps. Those who lived in the river parishes, for example, differed from the prairie dwellers. Slavery was more common in the river areas and the people in these sections displayed a different attitude toward the South's peculiar institution than did those Acadians who lived where slavery was less evident.

Of great importance to Brasseaux's work is the economic impact of the Civil War and Reconstruction on the Acadians. Southwest Louisiana experienced heavy fighting during the war that included the clash of rival armies and also the violent resistance of many Acadians to Confederate conscription. The resulting devastation combined with the economic upheaval that followed Appomattox to create a major economic crisis. Many Acadians who had enjoyed some antebellum economic and social success plummeted into the ranks of the poor and landless. These people became the Cajuns. So, too, did others who lived in the region and experienced similar hardships. Economically, socially, and culturally, all became Cajuns.

Many of Brasseaux's chapters focus on this transformation. Others are essentially studies of the Acadian community during secession and the Civil War. His section on Acadians and the evolution of political violence in southwestern Louisiana during the mid-nineteenth century is especially well done. Although each chapter is illuminating, Brasseaux does not always integrate them. He also tantalizes readers with the observation that African Americans contributed to Cajun culture and society, but he fails to elaborate.

Brasseaux supports his arguments with numerous statistical tables as well as solid research in available sources. The result is a carefully reasoned study that contributes greatly to an understanding of Louisiana Acadians. Brasseaux will undoubtedly address some of his unanswered questions, such as the matter of the African-American contribution to Cajun culture, in future works. These efforts will be welcome to students of Louisiana history and American ethnic minorities.

EDWARD F. HAAS
Wright State University

LINDA A. MORRIS. *Women's Humor in the Age of Gentility: The Life and Works of Frances Miriam Whitcher*. (New

York State Studies.) Syracuse, N.Y.: Syracuse University Press. 1992. Pp. xii, 253. \$32.50.

As Linda A. Morris points out, women (especially feminists) are often accused of lacking a sense of humor. It is important for accuracy, therefore, to identify the nature of women's humor in general, and to show it has survived in a separate literary tradition in the United States since the early nineteenth century. In writing this biography of Frances Miriam Whitcher, therefore, Morris is not only highlighting a nineteenth-century woman writer but also tracing a crucial early development in the American female vernacular humor tradition.

Vernacular humor emerged prominently in the 1830s in both the Northeast and old southwestern United States. It was the first humor to be "distinctively American in language, spirit, and subject" (p. 2). Such humor featured a relatively uneducated protagonist, one whose values often were opposed to those of the dominant culture. Speaking in a distinct dialect and using an "earthy" idiom, the comic hero often gave an uneducated but common-sensical interpretation to both people and events. The particular author was better educated than the character, but used the comic hero to state often unpleasant satiric truths.

As Morris points out, the vernacular humor tradition spawned two parallel lines: the American male and female vernacular protagonist. The female tradition addressed specifically female concerns (often domestic), featured first-person narration, and ultimately produced writers such as Marietta Holley, Gail Hamilton, Fanny Fern, and ultimately moderns like Betty MacDonald and Erma Bombeck.

Although Morris's literary-critical biography helps us understand the nature of the vernacular humor tradition among women, it is less useful in untangling an individual artist's (such as Whitcher's) artistic intent because Morris fails clearly to distinguish between comedy, satire, and farce. Additionally, Morris makes some claims for her female vernacular writers that are perhaps overstatements. Most comedy, for example, "is highly subversive" (p. 10) and "subverts" the values of the dominant culture; this is not unique to women's humor. Many of the points Morris makes are equally true for men's humor and sometimes are much more appropriate to a discussion of satire.

Despite this, Morris's biography of Whitcher provides a detailed picture of life for a woman in the American Northeast during the 1830s and 1840s, especially in the so-called "Burned-Over District." Through the comic reflections of Whitcher's "fool," the Widow Bedott, we encounter the silliness of female middle-class gossip, marriage practices, and life in a small town. The "Widow" sketches, published in the mid-1840s in Neal's *Saturday Gazette*, were instantly successful; her later work, the "Aunt Maguire" series, which seemed much more satiric in intention and much more thinly disguised (based on life

and people in Elmira, New York), was also extremely popular. The Aunt Maguire sketches appeared regularly in the late 1840s in *Godey's Lady's Book* and proceeded to mock the pretensions of many newly middle-class people to "gentility."

Finally, via the medium of biography and through generous quotations taken from Whitcher's vernacular heroine-narrators, Morris allows modern readers imaginatively to take a sewing circle seat and become participants in the world of nineteenth-century women's upward social mobility, as well as helping them to identify its more amusing foibles.

FRANCES B. COGAN
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KATHLEEN D. MCCARTHY. *Women's Culture: American Philanthropy and Art, 1830-1930*. Chicago: University of Chicago Press. 1991. Pp. xvii, 324. \$29.95.

Although American women have traditionally been considered the guardians of the nation's culture, little historical attention has been devoted to the specific contributions they have made to the establishment of museums, galleries, and other art institutions and organizations. Kathleen D. McCarthy's pioneering study of women's varied roles in the promotion of the visual and decorative arts in nineteenth and early twentieth-century America both fills this gap and addresses related issues concerning the gendered nature of American philanthropy. Providing a complex and nuanced reading of the interplay between philanthropy, gender, professionalism, and institutions, McCarthy explores the shifting relationships and strategies used by women who sought influence and power in an arena that, contrary to public perception, was controlled by men. Her book stands as a major contribution both to the histories of American art and cultural philanthropy and to the developing arena of gender studies.

McCarthy outlines three main philanthropic strategies employed by women who collected and encouraged the creation of art. The first, separatism, characterized antebellum and Gilded Age female entrepreneurs like Candace Wheeler, who founded the decorative arts movement (the first major artistic crusade among female control). Paralleling the efforts of female philanthropists who created nursing schools and women's hospitals after the Civil War, separatism helped professionalize women's artistry by feminizing the notion of the "decorative" arts (especially embroidery, ceramics, and statuary) as distinct from "fine" arts, which remained a male preserve. In so doing, however, separatist leaders relegated women's work to subordinate status.

The second strategy, assimilation, embodied similar contradictions. Working within male-dominated institutions, painters and patrons such as Mary Cassatt and Louisine Havemeyer, both feminists, strengthened collections in major museums that gave

little support to women as artists, curators, or trustees and that continued to define "fine" art as inherently male.

McCarthy considers the third approach, that of "individualists" like Isabella Stewart Gardner, Gertrude Vanderbilt Whitney, Gertrude Stein, Katherine Dreier, and Peggy Guggenheim more successful in combating deeply entrenched barriers to women's participation in the art world. Challenging separate women's institutions as well as established male museum models, their highly personal philanthropy encouraged new directions in collecting—especially avant-garde modernism and contemporary art—and led to the founding of pioneering institutions. Although their individualistic, antibureaucratic approaches, particularly their feminist agendas, were difficult to institutionalize, McCarthy feels these women left an extraordinary legacy that shaped the future course of modern art.

This broad narrative of philanthropic endeavor is interwoven with the absorbing personal stories of leading female activists and set against a backdrop of larger cultural change that characterized an increasingly interconnected network of American museums, foundations, and research universities. In showing us the gendered nature of such developments—especially how the creation, collection, and commodification of art was experienced differently by men and women—McCarthy demonstrates the vital link between art and power that stymied women's progress in their presumed "sphere" for at least several generations. Her book deserves to be widely read and discussed.

JOYCE ANTLER
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TERESA ANNE MURPHY. *Ten Hours' Labor: Religion, Reform, and Gender in Early New England*. Ithaca: Cornell University Press. 1992. Pp. xii, 231. \$29.95.

Labor historians need not descend into discourse to explicate the languages of working-class protest. In this crisply written study of the New England Association of Farmers, Mechanics, and Other Workmen of the 1830s and the New England Workmen's Association of the 1840s, Teresa Anne Murphy contends that artisans and operatives, the women of Lowell as well as the men of Fall River, refused "to cede control of religion and moral reform to the middle class" (p. 5). She challenges three interrelated interpretations: that evangelical Protestantism reinforced ruling-class hegemony and substituted for working-class militancy; that republicanism, rather than religion, provided the terms for working-class women to speak in public; that labor reformers, who organized by community rather than trade, were conservative and backward-looking. Rather, she concludes, labor reformers prefigured the future "as they transformed the discourse of personal reform

into an instrumental language of labor activism" (p. 190).

Shared culture and language did not reduce to identical meaning. New England workingmen undermined the paternalism of the region's visible capitalist class by showing how the factory system disrupted men's protection of the family and women's performance of domestic duties. Overwork interfered with religious devotion and self-improvement. Gaining moral autonomy from evangelical Protestantism, working people negated the authority of elites whose upholding of traditional community values appeared as a sham next to the degradation of wage labor. They used religion to critique industrialization. But the contradictory legacy of popular religion emerges from Murphy's fascinating discussion of factory revivalism whose challenge "was primarily a spiritual one" (p. 88).

Moral reform further questioned elite authority. In having the drunkard become the narrator of his own tale, Washingtonians reasserted men's rightful place as household heads. Whereas wives formed supportive auxiliaries, wage-earning women found their own voice through the languages of religion and reform. During the mid-1840s, they drew on notions of female morality and subverted understandings of domesticity to claim rights as women as well as workers.

Gender analysis enriches Murphy's contextualization of discourse. Workingmen exhibited their own paternalism in championing internal mechanisms to maintain children's contribution to the family at a time when dependents were becoming economically independent. Artisans made reduced hours the goal of labor reform rather than the higher wages sought by the less skilled. In the 1830s, they limited the leadership of wage-earning women; "proper female behavior" demanded male spokesmen. In embracing legislation to protect women and child workers, they reasserted their social roles as men. But the "daughters of freemen" linked respectability with independence rather than protection. The same act, like petitioning, "accommodated" differences between wage-earning women, working-class wives, and workingmen.

Only in discussing the rights talk of Lowell women does Murphy problematize the division between public and private that she uncritically employs elsewhere to explain differences by sex and kinship position. Greater attention to employer voices also would delineate better the common culture whose meanings working people sought to control. Murphy's strengths make one wish that this had been a more expansive study.

EILEEN BORIS
Howard University
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University of Helsinki

WALTER LICHT. *Getting Work: Philadelphia, 1840-1950*. Cambridge: Harvard University Press. 1992. Pp. xiii, 317. \$39.95.

This book represents an important departure from labor history's traditional focus on workers on the job or in the community, for it asks how wage earners and lower-level salaried employees got work. It thus addresses a problem long associated with labor economists in the neoclassical tradition who, as Walter Licht reminds us, have been content to assess the interplay between supply and demand. Licht makes a convincing case for the inadequacy of such an approach and for a broader, more synthetic perspective that deals with the relationship among "general economic conditions, personal means and decisions, agencies with the labor market, firm strategies, and state policy" (p. x). He draws on an impressive array of secondary works in numerous disciplines, and from a rich store of business records as well as census tracts and government reports, the most significant of which is a survey of the career histories of 2,500 Philadelphia workers commissioned in 1936 by the Works Progress Administration.

Licht uses the survey as a thematic guide and point of departure for probing the forces that shaped the search for work. He underlines the survey's major findings that personal initiative (40 percent) was the most effective means of job procurement, followed by family ties (27 percent), and personal connections (25 percent). Formal agencies of various kinds (9 percent) were a distant fourth. In separate chapters on schools, agencies, firms, government, and family, Licht systematically pursues the forces behind this pattern. We learn, for instance, that vocational education did not have much of an impact on the employment prospects of industrial workers, but that commercial education in parochial schools did prepare young women for clerical work. Apprenticeship programs started by businesses and unions proved to be the first rung on career ladders in industry for young men, as long as they were white. Racist employers and employees kept shop floors lily white through the Depression and New Deal. Employment breakthroughs for African Americans awaited the post-World War II era, when political pressure exerted through civic groups and government helped loosen job markets. This African-American experience was unique but also illustrative of Licht's larger point that labor markets are not hermetically sealed institutions governed solely by economic law. Instead, they are constituted by social relationships and conditioned by economic, social, and political forces. In the final analysis, the job seeker got the best results from personal initiative given the proprietary enterprise that was Philadelphia's industrial trademark.

Licht ranges widely across the city's institutional landscape, pausing here and there to offer arresting insights that fly in the face of received wisdom. He takes issue with some Marxists and most functionalists

who envision the public schools as training grounds for industry or agents of industrial discipline. Proponents of vocationalism in the public schools were thwarted by institutional inertia, localism, and a democratic spirit, not to mention working-class children themselves, who by necessity left school early to help support families. The notable exception was Catholic education. Private and hierarchically arranged, parochial schools got around the forces that stifled innovation in the public sector and developed a strong vocational curriculum, especially for young women bound for office work. Local factories, moreover, were not the bureaucratized, vertically integrated corporations depicted in the secondary literature, both Marxist and non-Marxist. As late as the 1920s the vast majority of firms in manufacturing and services did not even have personnel departments, which left hiring, firing, and promotion in the hands of lesser managers and supervisors. More surprising still is Licht's discovery that recognizably modern personnel policies emerged first and most fully in the public sector with the development of the civil service system. So much for the widely held assumption that the private sector had (and continues to have) a greater affinity for meritocratic personnel practices than the public sector.

This is not to suggest that Licht has written a flawless study. He overlooks the workings of internal labor markets as well as the means of wage and salary determination, and does not quite explain why certain ethnic groups came to dominate particular jobs. Also, his book's last two chapters, on government and family strategies, are a bit long-winded and somewhat diffuse. But such quibbles should not deter labor specialists or generalists, or indeed anyone interested in sensible public policy. All will learn from this original work by a mature, independent-minded scholar. We owe it to him and ourselves to see if his intriguing claims hold for other industrializing centers.

BRUCE LAURIE
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RONALD J. ZBORAY. *A Fictive People: Antebellum Economic Development and the American Reading Public*. New York: Oxford University Press. 1993. Pp. xxii, 326. \$45.00.

A comprehensive, innovative, and timely study, Ronald J. Zboray's impressive book sets a new standard for *histoire du livre* explorations in the American context. Challenging recurrent assumptions about a direct link between antebellum economic growth and the democratization of literary culture, Zboray demonstrates the tangible limits on the social institutions and indicators—high literacy rates, improved printing technology, the development of common schools and lyceums, even the cult of domesticity—customar-

ily credited with inaugurating a golden age of reading. The real strength of this analysis, however, lies in Zboray's case studies of specific publisher habits and reader habitats. Whether retracing the traveling circuit of book peddler Mason Locke Weems, deciphering the layout of an antebellum bookstore, or tracking circulation records (by gender) for the private New York Society Library, Zboray's book establishes itself as an authoritative account of a decidedly segmented reading populace.

In Zboray's title, the words "fictive people" function primarily in two ways. First, following John Higham's hypothesis about the movement from antebellum cultural "boundlessness" to "consolidation," Zboray emphasizes the new ways in which readers, set loose from premodern communal norms, fictively constructed their identities in a post-Constitutional era. Second, Zboray uses the term to underscore the fact that, despite technological and marketing advances, a truly national reading public was largely a phantom of publishers' imaginations. Especially in recent literary histories intent on delineating the complicity of even Romantic protest with nascent industrial capitalism, declarations about the arrival of a "mass literary marketplace" in the 1850s are legion; Zboray's study will, therefore, provide a much-needed corrective. Zboray's analysis shows distribution networks still differentiated by region, consumption patterns still stratified by class, preferences still colonial and eclectic, and, perhaps most notably, readers' tastes not as radically divided along gender lines as has been supposed.

Of course, the challenge has been to link such materialist analysis to antebellum readers' appetite for the seemingly immaterial, that is, their growing taste for sentimental and historical fiction. In this instance, however, Zboray's dependence on Higham's thesis serves him less well. The familiar tradition/modernity dichotomy, yoked to somewhat grand hypotheses about "a process of obliterated self-hood" (p. 99) through modernization, tends to needlessly nullify the segmentation emphasized by Zboray's own local groundwork. Meanwhile, absent detailed textual readings, assertions that fiction "largely ignored local sensibilities" (p. 82) or constituted an "emotional" reading experience seem alternately undersubstantiated and overgeneralized. One problem is that Zboray is still bound to the traditionally derogatory assumptions about romance, a tendency that breeds an implied declension thesis: as American audiences stray from rationalism, the promise of American boundlessness becomes cultural "chaos" (p. 194). Rightfully intent on setting aside the sweeping historical generalizations plaguing literary histories, Zboray may not have absorbed all the lessons this scholarship—by Cathy N. Davidson, Jane Tompkins, Richard Brodhead, and Michael Denning, among others—has to teach about the cultural poetics of genre conventions and the complexities of readerly engagement. Nevertheless, that scholarship will also now

have to take account of Zboray's findings. In this way, he is to be congratulated for creating a book that transforms our reading.

CHRISTOPHER P. WILSON
Boston College

GERALD J. BALDASTY. *The Commercialization of News in the Nineteenth Century*. Madison: University of Wisconsin Press. 1992. Pp. xii, 227. Cloth \$60.00, paper \$19.95.

The title of this slim volume fairly well conveys its theme. Gerald J. Baldasty is concerned with a profound change that came over American journalism during the nineteenth century. Through the Jacksonian era and after, editors defined news primarily in political terms and saw it as an instrument to serve party interests. By the late nineteenth century, business considerations prevailed, and the definition of news hinged on what would ensure or increase newspaper revenues. Which is to say, in the course of the century news became commercialized.

Baldasty is on safe ground in tracing this transformation to three interrelated developments. Partly it had to do with advertisers replacing political parties as the chief financial backers of the press. When that happened, loyalties and perspectives shifted. Whereas editors had once defined the news with the interests of a political constituency foremost in mind, now the interests of a business constituency took priority. Editors also started to see their readership in a new way. Partisan editors had addressed readers primarily as voters, but commercialized editors learned to think of them primarily as consumers. Not least in encouraging the trend toward news being shaped and packaged with an eye toward profit was the fact that journalism itself became a big business. By the end of the Civil War it required an estimated one million dollars in start-up capital to establish a newspaper in New York City, a far cry from the \$500 James Gordon Bennett needed to launch his *New York Herald* in 1835. Almost inevitably, with heavy overhead and operating costs went a ledger mentality in defining what newspapers were all about.

Baldasty develops these themes in a nicely written and exhaustively researched monograph. He makes some interesting points. One example is his account, based on a study of all English-language newspapers in California and New York in 1899, of how population base influenced the political affiliations of the commercialized press. Small weekly newspapers, which could not afford to antagonize any potential subscribers, tended to be independent politically, often even refusing to endorse candidates in local elections. Partisanship was more common among newspapers in cities and large towns (particularly those located in county seats where patronage printing contracts were available). The larger pool of potential readers gave greater scope to take a stand.

The problem with the monograph is that exhaustive research does not necessarily lead to new insights or even striking new information. By and large, Baldasty tells us what we already know. The progression from the journalism of Duff Green to that of Joseph Pulitzer is hardly a new theme in the history of the American press. And Baldasty's explanations for why the progression occurred, while sensible, are still the familiar ones.

Still, this is a nice overview of an important subject, although perhaps a bit overly pessimistic in evaluating the consequences of commercialized news. One might argue that whatever the merits of the nineteenth-century partisan press, it contributed little to reasoned or mature political discourse. And whatever the failing of the commercialized press, it at least gave us a few of the finest newspapers this country has ever produced.

GEORGE JUERGENS
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T. WALTER HERBERT. *Dearest Beloved: The Hawthornes and the Making of the Middle-Class Family*. (The New Historicism, number 24.) Berkeley and Los Angeles: University of California Press. 1993. Pp. xx, 331. \$28.00.

T. Walter Herbert's book is a brilliant study of the relationship between Nathaniel Hawthorne's literary work and his tumultuous domestic life. Herbert contends that Nathaniel and Sophia Hawthorne attempted to realize the domestic ideal of family relations that became dominant in the nineteenth century. He describes how their marriage, which began with sexual and spiritual harmony, became a battleground marked by agony, madness, and estrangement. He argues persuasively that "Hawthorne's writing explores the entanglement of misery and beatitude that was native to the domestic ideal—the morbidity intrinsic to its health, the sorrow inseparable from its joy, the disorder native to its harmonies" (p. xvi).

The book is divided into four parts. The first part begins when Nathaniel Hawthorne was entering a period of psychic crisis generated by familial conflict. The second part focuses on the social origins of the Hawthornes' marital dilemmas and vividly traces their psychosocial development. Part three explores the psychic interior of the Hawthornes' domestic sphere. In one of his most imaginative sections, Herbert analyzes *The Scarlet Letter* as an exploration of the joys and torments of domestic intimacy. In the fourth part Herbert describes a stressful period in Rome where Hawthorne suffered anguish over his daughter Una's madness, his conflicts with Sophia, and a scandal involving his friend Louisa Lander, an American sculptor.

Herbert explores the emotional consequences for

the Hawthornes of trying to conform to the rigidly gendered roles of middle-class domesticity. He characterizes Sophia as the quintessential "angel in the house" who professed absolute devotion to her husband but ultimately revealed her own will to power in repressed rage. Likewise he describes Nathaniel's anxieties over his own gender identity in his attempts as a writer to become a self-made man in an increasingly commercial society.

This book is a splendid example of the new historicism. Scholars who adopt post-structuralist approaches will be dismayed by Herbert's unabashed claims regarding Hawthorne's life and literary works. Moreover, while Herbert openly admits that "orthodox Freudian theory is individualist and ahistorical in a fashion unsuited to understanding the marital politics of the Hawthornes's household," Freudian theory hovers throughout the book (p. 112). Generally, Herbert deftly uses psychoanalytic theory. At times, however, he seems to go beyond the evidence. For example, his interpretation of Hawthorne's "revulsion" at the "sexual" antics of his daughter Una seems to be overdrawn (pp. 178–81). Moreover, with slim evidence he argues that Hawthorne experienced anxieties about the virginity that males should retain in relation to other males (p. 262). He also invests symbolic meaning in Hawthorne's persistent desire to kiss Sophia's nose, "a phallic image," and her reluctance (p. 122). These minor instances of lapsing into "orthodoxy" aside, his interpretation is imaginative and insightful.

Herbert's study is a major contribution to the field of scholarship on Nathaniel Hawthorne's fiction and his life, and it sheds light on the history of women and the middle-class family in the nineteenth century. A major strength of the book stems from Herbert's extensive use of archival sources, including the Hawthornes' personal letters and journals, the centenary edition of all of Nathaniel Hawthorne's literary works, and a diverse array of secondary works. Written in an engaging style, the book presents a provocative interpretation of the Hawthornes' reciprocal rage that was generated by repressive gendered familial roles and the perennial struggle to harmonize individuals' needs for both achievement and intimacy.

CAROLYN JOHNSTON
Eckerd College

CHARLES CAPPER. *Margaret Fuller: An American Romantic Life*. Volume 1, *The Private Years*. New York: Oxford University Press. 1992. Pp. xv, 423. \$39.95.

In this extraordinarily well-written first volume of a projected two-part biography, Charles Capper bids fair to revivify the life-and-times model of intellectual history and, by interweaving Margaret Fuller's writing and experience with that of the men and women with whom she interacted, to expand the scope within

which nineteenth-century women's lives have generally been reconstructed. Lucky in having chosen a subject most of whose voluminous correspondence and journals have been preserved, Capper draws also on similarly extensive manuscript collections left by the self-consciously introspective group of Yankees whose literary, philosophical, and religious writings demonstrate the intellectual concerns and illuminate the social dynamic of Fuller's world. Yet, despite the tempting potential for digression, Capper keeps his focus on Fuller. He confirms the familiar Margaret whose scathing repartee, arcane literary style, widely perceived arrogance, and dark moods conform to the stereotypes of aspiring bluestockings embittered by external limits on their lives. But he also introduces a largely unfamiliar Margaret whose wit and humor, joy in the friendship of her seniors, and enthusiasm for teaching and mentoring her peers and juniors—whether male or female—made her a valued guest, an engaging conversationalist, and a favorite friend.

This intricate exploration of personality through the lens of Fuller's relations with others in her circle allows Capper to re-create the social medium in which the cultural phenomenon of Transcendentalism took shape. As they read large sections of each others' journals and sent to one subset of friends the intimate letters circulated among members of another, these mostly young men and women intertwined personal emotion with intellectual probing. Given that mixture, it is no surprise that Capper informs his study with feminist and psychoanalytical literature. Yet, except for a few footnotes and an occasional unnecessarily elaborate explanation, his work is no stumping ground for psychologizing or feminist theory. It is Fuller's own words, written several years before *Woman in the Nineteenth Century*, that spell out her dilemma about gender roles. At age thirty, she wrote, "One should be either private or public. I love best to be a woman; but womanhood is at present too straitly-bounded to give me scope. At hours, I live truly as a woman; at others, I should stifle; as, on the other hand, I should palsy, when I would play the artist" (p. 338).

Indeed, how Fuller handled the choices she had to make and how Capper reconciles her often conflicting self-portrayals mold much of his interpretation. For instance, he deals with Fuller's factually inaccurate adult evaluation of her life as a child prodigy not as a distorted depiction of her father's determination to cultivate his daughter's mind and ignore her need for praise or play but as her informed reflection on a complex relationship. Other apparent conflicts, however, are less easily resolved. Do Fuller's assumption of family responsibilities after her father's death and the demands of her subsequent teaching ventures explain her failure to write the long-planned and extensively researched biography of Goethe she had made the centerpiece of her literary studies? Or did she use self-created pressures on her time to avoid a

task she feared to undertake? Perhaps less a matter of choice was the androgyny that fed Fuller's intense friendships with Caroline Sturgis and Anna Barker, which Capper believes differed markedly from commonplace "sentimental female friendship" (p. 282), and her explicit wooing of two men, each of whom rejected the love she proffered. Her assertiveness in all four cases provokes reflection on Capper's interpretation (p. 288) of her observation in 1839 that "I think [it] perfectly true . . . that women who love and marry feel no need to write. But how can a woman of genius love and marry?" as the first sign of an emerging feminism.

In his frequent but judicious use of extensive offset quotations, Capper actively engages the reader's judgment without creating the repetitious tedium this device customarily produces. Moreover, the juxtaposition of interrelated but distinctive observations allows him to trace Fuller's role in the evolution of Transcendentalism within the broad discourse on religion, education, and the arts on which it built. Initially confined to liberal Unitarian ministers seeking a religious belief system that would substitute Romantic intuition for the conservative rationalism of their elders, but that would encompass as well the scientific findings of German biblical scholars, the Transcendental Club soon expanded to include non-clerical reformers and literati. Fuller, the only woman to participate regularly in its meetings, gradually reshaped its focus until, when she was chosen to edit the *Dial*, she filled the first issue with cultural rather than philosophical articles after its originators had declined to write for it. To that, as to subsequent issues, she brought the fruit of her *Private Years*: the extensive classical learning inculcated by her father, the study of European—especially German—Romantic literature begun during her adolescence in Cambridge, the practical pedagogical skills and reform enthusiasm first acquired in Bronson Alcott's Temple school, and the social and intellectual sophistication that came from two years of conducting her famed *Conversations*—essentially collegiate education for women. Thus, although this first volume deals with Fuller before the *Dial* began publication in July 1840, before she became a nationally known journalist, and before she took her stand with European socialists and Roman revolutionaries, it is far more than an exploration of the obscure years in the life of a woman who later achieved notoriety. If Capper's second volume, on Fuller's public years, equals his first, he will have produced not only a definitive biography but also a broadly conceived addition to American intellectual and social history. And he will come close to answering the cogent question posed by a friend of the youthful Fuller (p. 314). "What a Sphinx is that girl! who shall solve her?"

JANE H. PEASE
College of Charleston

JOSEPH BRENT. *Charles Sanders Peirce: A Life*. Bloomington: Indiana University Press. 1993. Pp. xvi, 388. \$35.00.

Joseph Brent's splendid biography of Charles S. Peirce (1839–1914) dispels much of the mystery that has surrounded the difficult life and career of America's greatest philosopher. The refusal of Harvard's philosophy department to grant permission to quote from key documents, ostensibly to protect Peirce's reputation, prevented the biography's publication for over thirty years. Brent's biography details, at last, the extreme conditions under which Peirce wrote some of his most influential texts in philosophy, logic, and semiotics.

The general outlines of Peirce's life, checkered career, and long decline have long been known. Born into the intellectual and scientific elite of nineteenth-century America, Peirce early showed promise of becoming an important scientist and logician. In the 1870s, he became an internationally recognized scientist with the U.S. Coast Survey, began to teach at Johns Hopkins, and started to publish in logic and the scientific method. He also acquired the reputation of being difficult, moody, sometimes violent, and often irresponsible regarding deadlines and money. Between 1883 and 1891, Peirce divorced his first wife, married his second (a mysterious French woman who had been his mistress), was dismissed from Johns Hopkins, and, eventually, from the Coast Survey. Peirce spent his last decades in Milford, Pennsylvania, plagued by ill health and poverty and sustained largely by the charity of friends, including William James. In the midst of this personal decline, Peirce produced some of his most important work.

Brent's biography uncovers the likely sources of Peirce's misery. A lifelong sufferer from a painful hereditary facial neuralgia, Peirce often took drugs (including morphine and probably cocaine) to relieve the pain. Both the neuralgia and the drugs contributed to his difficult personal relations. Pampered and protected as a youth and something of a dandy, Peirce never learned the self-control and sense of responsibility demanded by contemporary academic and scientific institutions. But Peirce's downfall was not solely attributable to his character flaws or disease. Brent paints an unflattering picture of certain moralistic scientists and academics, particularly Simon Newcomb and Charles W. Eliot, who were unable to grasp the significance of Peirce's intellectual achievements and who repeatedly used his character flaws to deny him a permanent position commensurate with his abilities.

The man who emerges in these pages is difficult to like. Changeable, arrogant, irresponsible, and often self-destructive, Peirce challenged anyone's friendship. There also emerges, however, the portrait of an individual totally dedicated to the pursuit of method and truth as he conceived it, even at the cost of comfort and health.

This fascinating and well-written book immediately becomes the standard biography of Peirce. Although the focus here is on Peirce's life, Brent effectively integrates a discussion of the major developments in Peirce's thought into the text. His explanations of the philosophical, logical, and scientific writing are generally clear and convincing, if not always easily accessible to those with no prior familiarity with Peirce's work. The Harvard philosophers were wrong; this excellent biography can only increase respect for and understanding of Peirce's intellectual achievement.

DANIEL J. WILSON
Muhlenberg College

ANDREA MOORE KERR. *Lucy Stone: Speaking Out for Equality*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. 301. Cloth \$43.00, paper \$15.95.

Lucy Stone is one of the more neglected icons in the feminist pantheon. Andrea Moore Kerr has sought to rehabilitate Stone's image, revealing the reformer in all of her complexity. She was an extraordinary woman. From a grim childhood on a Massachusetts farmstead she managed to put herself through Oberlin College, becoming a prominent lecturer for abolitionism and women's rights at a time when few women had the courage to take to the public platform. In 1869, she was one of the guiding spirits behind the creation of the American Woman Suffrage Association, counterpointing Elizabeth Cady Stanton's and Susan B. Anthony's more radical suffrage campaigns.

Kerr's biography provides a sympathetic narrative of Stone's public career, as well as her private woes. Initially committed to remaining single, she was eventually wooed and wed by the charming, irresponsible Henry Blackwell. Blackwell's role in the women's movement has often been recorded, but Kerr's insights into his human failings are both new and revealing. Insecure and an inveterate wastrel, he eventually reneged on many of his prenuptial promises, manipulating Stone into a far more conventional marriage than she had initially envisioned. He also squandered her earnings on a spate of unsuccessful business ventures, from real estate speculation to beet farming. Nor were his sisters (including the medical pioneer Elizabeth Blackwell) particularly sympathetic to Stone's proto-feminist crusades.

These details are important because they provide fresh insights into the public and private constraints that enveloped Stone's career, highlighting the intensity of her commitment. Kerr is particularly informative about her split with Stanton and Anthony, painting these revered radicals with an extremely critical brush. In the process, Kerr's analysis captures the acrimony as well as the ideological zeal that surrounded Gilded Age suffrage campaigns.

The book is strongest on the personal level, following the course of Stone's marriage and career in great

detail. The narrative of Stone's personal life, however, is weakened by Kerr's unwillingness to turn "the lens of popular psychology" (p. 1) on Stone's relationship with her alcoholic father. Several aspects of Stone's personality are striking: her unquestioning willingness to take responsibility for others' actions (including the financial disasters of her spendthrift husband); her "workaholic" nature; her low self-esteem; her desire for control. All are also classic earmarks of the child of an alcoholic. Many biographers have stressed the importance of paternal relationships in shaping women's public careers; one wishes that Kerr had done more with this theme.

The book would also have benefited from a keener sensitivity to the social context in which Stone executed her campaigns. For example, Kerr notes that in the 1850s "Stone was earning between \$500 and \$1000 per week" through lecturing fees (p. 74). This was an enormous sum in a period when the annual laborer's salary hovered around \$600. Was it common for antebellum female reformers to earn between \$25,000 and \$50,000 per year? In many respects, Stone was a highly successful entrepreneur, a theme that should have been developed more fully.

The chapters that work best contextually are those that deal with the history of the American and National Woman Suffrage associations, including the increasingly bitter relationships between Anthony, Stanton, and Stone. In Kerr's interpretation, Stanton's and Anthony's duplicitousness, rather than Stone's conservatism, kept the two associations apart. For readers interested in Stone's turbulent personal life, Kerr's book provides an invaluable narrative; for those interested in the Gilded Age suffrage movement, it illuminates the fissures that riddled the *fin-de-siècle* women's movement in new ways. As such, it deserves an ample audience.

KATHLEEN D. MCCARTHY
Graduate School and University Center,
City University of New York

JOHN CALDWELL GUILDS. *Simms: A Literary Life*. Fayetteville: University of Arkansas Press. 1992. Pp. xiv, 426. \$35.00.

Despite the efforts of eminent literary and cultural historical critics such as Vernon L. Parrington, William R. Taylor, Lewis Simpson, and Louis Rubin, most literary critics ignore the work of William Gilmore Simms, the antebellum South's most important man of letters. After more than thirty years of research on Simms, John Caldwell Guilds believes he is able to explain that neglect and resurrect that literary reputation. Privy to materials few scholars have seen, he has filled many gaps in Simms's life and creative output. Guilds has explicated Simms's writings and carefully critiqued the reviews of Simms in the major journals and newspapers of those times. He asserts that Simms remains neglected because he was on the

wrong side during the Civil War and because he was a most difficult person in his private and public relationships. Guilds concludes that Simms should be studied today because of his realistic novels, excellent literary criticism, and major contribution to American letters.

Those who have read and studied Simms certainly agree with Guild's reasons for wanting to bring his work and life to the attention of modern scholars. Guilds elaborates on his defense of Simms's worth as a writer through detailed commentary and cogent suggestions about the fiction, poetry, and literary and political criticism. He makes a most perceptive critical and nuanced analysis of Simms's revolutionary romances, much-neglected novels of the pre-British colonial settlement of the southern region, and border tales. Guilds shows Simms's brilliant depiction of the clash of politics and class conflict that led to internal civil war both in revolutionary South Carolina and on the southwestern frontier. In his comments on that historical fiction, he discusses the importance of Simms's emphasis on historical setting and the values, personal qualities, and tensions among Simms's most important characters.

Guilds also takes pains to understand Simms the man. His is a sympathetic but truthful view of a writer misunderstood by both friends and later critics. He sees Simms as an indefatigable worker despite the security of his plantation, sensitive to slight despite his high place in South Carolina society, and an obsessed family man despite sometimes neglecting his wife and children. Guilds shows that Simms's own claims of being misunderstood and ignored led his first biographer, William P. Trent, to claim him to be a victim of the South itself. Guilds justifiably and convincingly disputes Trent's image of Simms and replaces it with that of a classic complainer but an enormously talented and well-received antebellum writer who contributed positively to American letters.

If there is criticism of Guilds's efforts to resuscitate the artist and the man, it is that in his attempt to make him an American writer and chronicler of the American experience he ignores Simms the southerner. Guilds's comments on Simms as historical analyst actually shows that Simms's subject always was the evolving South. Even Simms's best work, the border romances, described the clash of barbarism and civilization on the southwestern frontier. Simms also interacted with many of the Old South's most important writers. Guilds ignores Simms's relationships with southern writers such as John P. Kennedy and Richard H. Wilde and the literary culture that nurtured and influenced them. Simms was a planter slaveholder, but slavery is only barely mentioned in Guilds's book. That Simms rarely wrote on slavery, except to defend it, requires at least some explanation in the evaluation of artist and man. In short, Guilds discounts the southern setting that made Simms. But certainly it is in part the tensions in Simms's own work about the South that demands that modern critics

study this enormously creative and perceptive literary artist.

JON L. WAKELYN
Catholic University of America

MICHAEL BROYLES. *"Music of the Highest Class": Elitism and Populism in Antebellum Boston*. New Haven: Yale University Press. 1992. Pp. ix, 392. \$32.50.

Michael Broyles seeks to explain a fundamental duality in American musical culture: the distinction between what is termed classical music (music that in the early nineteenth century came to be viewed as superior and edifying) and music that is considered merely entertaining. The former, based on European art music, contained more than an aura of respectability for elitists of antebellum America; in symphonic works especially, such pieces could be used for moral enhancement. Broyles holds that American musical culture developed a unique profile resulting from cultural forces, intellectual traditions, and political beliefs that were distinctive to the American experience.

Boston, the author maintains, provided an ideological framework accepted by the rest of the country during the middle decades of the nineteenth century. The journals of the Transcendentalists did much to spread an idealistic purview of music and establish a musical hierarchy in America. Later in the century critic John S. Dwight had difficulty extending his pantheon of composers beyond Beethoven; even Brahms troubled him, and he flatly refused to recognize Wagner. But Broyles traces the American premise that certain types of music were morally superior to other kinds back to the reform movement that began in the 1720s and attempted to impose singing by rule on Boston's congregations.

Broyles illustrates the continuing importance of the sacred music reform movement to the country's musical development by focusing on the career of Lowell Mason, the noted hymn composer and pioneer in the field of music education, active in Boston from 1828 to 1845. The European orientation of Mason's psalmody eradicated an indigenous American music, exemplified in the fugues of William Billings, while his educational philosophy fostered a stilted atmosphere that placed correctness over originality. Education became fundamental to the cultivated tradition in America; Mason's goal was to instill respect for compositions that were restrained, chaste, and dignified.

Although Boston's upper classes remained slow to include music in their cultural stewardship, the city's Handel and Haydn Society, predominantly middle-class, evolved into a durable concert organization at a time when concerts generally proved risky ventures. By 1835 the Boston Academy of Music spearheaded a movement favoring instrumental music, gaining the support of the wealthy, but stratifying the city's musical activities in the process. Opera took longer to

attract the backing of Boston's upper and middle classes, since lyric drama was tainted by its association with the theater. Toward the end of the 1840s music in America was more polarized than ever before, with symphony orchestras and limited opera performances on the one hand and popular entertainment, led by minstrel shows, on the other.

Broyles contends that a duality exists still in American musical culture, although the ground under both poles has become less rigid. The bebop musicians of the 1940s, for example, were remote and contemptuous of audiences, characteristics these jazz artists shared with serious musicians of the previous century. More recently the Beatles, Cole Porter, Mel Tormé, Josh White, and Ella Fitzgerald have transcended entertainment to enter the realm of art.

This book is a significant study for American social historians as well as music historians. Broyles deals with church choirs, music in private homes, nocturnal serenades of young ladies, and military bands, but he also discusses the role of women as patrons of music and the rift between professional and amateur musicians. If he becomes mired occasionally in a morass of detail or prolonged technical analysis, his insights are rewarding. Broyles has produced a well-crafted monograph, placed musical developments within a rich historical context, and provided, for the most part, a readable narrative augmented by fresh analysis.

RONALD L. DAVIS
Southern Methodist University

THOMAS BENDER, editor. *The Antislavery Debate: Capitalism and Abolitionism as a Problem in Historical Interpretation*. Berkeley and Los Angeles: University of California Press. 1992. Pp. x, 325. Cloth \$45.00, paper \$14.00.

This is a book with narrow and broad constituencies, as well as expansive boundaries and clear limits. The narrow constituency is historians interested in antislavery prior to the 1830s. For them, the issues and most of the essays will be familiar. The broader constituency is scholars from any discipline interested in historical interpretation and practice. For them, the book is refreshing. Few debates among American historians range so well from nitty-gritty matters of quotations and evidence to fundamental questions about the relationship between consciousness and social, political, and economic change.

The book turns on the second volume of David Brion Davis's magisterial study of slavery, *The Problem of Slavery in the Age of Revolution* (1975), chapters from which comprise almost the first third of this work. In 1985, the *AHR* published a two-part essay by Thomas Haskell acknowledging Davis's accomplishment while suggesting an alternative formulation of the relationship between humanitarian sentiment and capitalism ("Capitalism and the Origins of the Humanitarian

Sensibility," 90 [April, June 1985], 339–61; 547–66). This essay, rejoinders by Haskell and Davis, and two pieces by John Ashworth, who entered the fray with an *AHR* article of his own in 1987 ("The Relationship between Capitalism and Humanitarianism," 92 [October 1987], 813–28), comprise the rest of volume. A brief "Introduction" by Thomas Bender summarizes the main points, properly situates the essays within "a general debate about historical explanation" (p. 2), and suggests how each position informs the others. His is the voice of judicious moderation, which, unfortunately, means most readers will concentrate on the rhetorical fireworks, especially in the later pieces, two by Ashworth and Davis previously unpublished, where the issues and tone become sharper.

Initially, the debate centered on Haskell's reading of Davis's interpretation, which Davis disputed and later either clarified or modified, depending on one's allegiances. Haskell faulted Davis's use of "hegemony," "interests," and "self-deception" to characterize abolitionism. He proposed a provocative relationship between capitalism and the rise of humanitarianism that posited a new cognitive style in which individuals could imagine remote moral connections and consequences unthinkable earlier. Ashworth, whose voice enters the debate after the book's midpoint, disputed Haskell's critique, posited yet another connection between capitalism and humanitarianism by way of wage labor, and expanded the horizon to encompass other reforms and a wider transformation of values.

The result is not necessarily a "victory" for any side, although Haskell presents the most intriguing, if least documented, suggestion, and Davis was compelled to explore and clarify the implications of his interpretation. At their best, the essays are a textbook analysis of how language, evidence, interpretation, and theory intersect in historical writing.

The debate, however, is almost as fascinating for what it excludes, or slights, as for its sweep. Gender appears sparingly, in spite of the vital role of women in abolitionism and differences in how capitalism and wage labor affected them. Capitalism remains a murky and, at times, ahistorical concept, particularly in Haskell's essays, as Ashworth and Davis note. Comparisons with market-oriented cultures that did not produce humanitarian sentiment—especially with the Dutch—are not fully explored. Finally, the theoretical positions evoked scarcely exhaust those held by present-day historians. Karl Marx, Max Weber, and Antonio Gramsci dominate the discussion. The only references to Michel Foucault come from Haskell, hardly a disciple.

Perhaps that is appropriate. The exhilaration in reading Davis, Haskell, Ashworth, and Bender comes from following first-rate historians talking seriously about their craft. But there is also a caution: this is not the only possible debate about historical causation and explanation, or about relationships between hu-

manitarianism and capitalism and consciousness and society. It is simply a very fine one.

RONALD G. WALTERS
Johns Hopkins University

SHIRLEY J. YEE. *Black Women Abolitionists: A Study in Activism, 1828–1860*. Knoxville: University of Tennessee Press. 1992. Pp. xii, 204. Cloth \$34.95, paper \$17.95.

Shirley J. Yee's study enriches our understanding of black women's experiences in antebellum America and makes a valuable contribution to other related fields of historical inquiry. With the publication of this book, black women's history now has two scholarly monographs for the period from 1619 to the Civil War, the other being Deborah Gray White's *Ar'n't I A Woman: Female Slaves in the Plantation South* (1985). Clearly the need exists for more studies in this area, especially works that follow the lead of Suzanne Lebsock and Elizabeth Fox-Genovese that compare the experiences of black and white women.

This thoughtful exploration moves black women such as Sarah P. Remond, Sarah Mapps Douglass, Mary Ann Shadd Cary, and Nancy Gardner Prince from the margins of the abolitionist movement and places them at the center of antebellum protest culture. Black women's activism was a complicated affair. They wedded the fight to end slavery and to eradicate negative racist stereotypes of their sexuality to the ongoing struggle for women's rights. Thus, black women's activism begs a nuanced understanding of multiple, frequently overlapping identities and responsibilities. Black women felt compelled to satisfy contemporary expectations of womanhood, perform racial uplift work, and to walk the "fine line of female 'respectability'" (p. 5).

To explicate a theme of multiple and conflicting responsibilities to both their gender and to their race, Yee organizes her study of black women's activism into six chapters, beginning with kinship, friendship, and community and ending with the sowing of the seeds of black feminism. The text emphasizes those relatively privileged black women who operated in well-known corridors of antislavery agitation, primarily the Midwest and the Northeast, and to a lesser degree in California and Canada. The antislavery or resistance work of slave women or of southern free black women awaits future investigation.

The intersection of race, gender, and class differences often isolated black women from their natural and traditional allies. In February 1832, a group of black women met in Salem, Massachusetts, to form the first female antislavery society as much to escape the racism of white women as to avoid the sexism of black and white men. Yee's splendid archival research and skillful mining of the secondary literature reclaims from historical obscurity an array of activist antebellum free black women. Moreover, Yee ex-

plores themes central to women's history, as exemplified in the chapter on black women and the cult of true womanhood.

Through persuasive analysis of the continuity of black women's community building, political networks, kinships, and friendships, Yee offers insight into their consciousness as "race women." In the conclusion she accurately connects the founding of the black female antislavery society in 1832 with the establishment at the end of the century of the National Association of Colored Women clubs. There is much to learn here. This important study should be required reading in black, women's, and black women's history courses.

DARLENE CLARK HINE
Michigan State University

LYNDA J. MORGAN. *Emancipation in Virginia's Tobacco Belt, 1850–1870*. Athens: University of Georgia Press. 1992. Pp. xiii, 329. \$45.00.

Like Joseph P. Reidy's excellent study of central Georgia (*From Slavery to Agrarian Capitalism in the Cotton Plantation South* [1992]), Lynda J. Morgan examines the transition from slavery to freedom over time and in a specific region. Her book on black life in Virginia's twenty-four tobacco counties focuses minutely on the role that African Virginians exerted on social and economic change from the decade preceding the Civil War through the end of Reconstruction.

In 1860, Virginia had the largest slave population in the South and the state's tobacco belt held almost one-half of Virginia's slave population and produced more than three-quarters of its cash crop. Whereas most blacks worked on plantations, a minority of slaves and free blacks, Morgan argues, experienced "wagelike labor" as hired workers in Virginia's eastern and urban "market economies." As a flexible, heterogeneous, and mobile labor force, hired slaves and free blacks gained opportunities for "independent production and exchange" and grasped the workings of "a market economy" (p. 3). These experiences, accentuated dramatically by wartime impressment and emancipation, revolutionized the "pre-capitalist African-Virginian social ideology" (p. 12).

Morgan provides insightful analyses of slavery in Virginia's southern piedmont: black kinship and family networks, the ominous interstate slave trade, slave resistance, slave agricultural and industrial hiring, and farm tenancy among free blacks. Drawing on an astute reading of the Southern Claims Commission Papers, she explains how some slaves acquired property. Her treatment of slave hiring—especially its elasticity—is the most thorough in print. Industrialists in Richmond, for example, hired slaves from the interior depending on their labor needs. Morgan explains that, ironically, during the Civil War slave hiring served to undermine master-slave relations

and, ultimately, "compromised the goals of the war" (p. 104).

She offers a penetrating critique of the Freedmen's Bureau in interior Virginia during Reconstruction, correctly interpreting it as a conservative force that "preached a new paternalism" (p. 134). Freedpeople reestablished families and built schools and churches as successfully as possible. Ultimately, Morgan writes, they came to consider land acquisition as their only true security. It "provided them with control over their own labor and its fruits, thereby allowing them to escape selling labor on the market" (p. 136). By the 1870s land, skills, "and to some extent antebellum experience with hiring" led to the emergence of a "nascent bourgeoisie" (pp. 163-64).

Given Morgan's proper emphasis on the diversity of black life in Virginia's hinterland, she underestimates the extent to which blacks in Virginia's tobacco belt—not just hired or skilled slaves and free blacks—understood market economic forces. The author correctly notes that "Rural plantation slaves . . . shared an essentially precapitalist social outlook that had been leavened with some intelligence about the market, the result of the changing economy in which they lived" (p. 33). She concludes that slaves learned "individual bargaining methods suggestive of free labor methods" and maintained "strong preindustrial desires for landholding and independence" (p. 227). Perhaps, then, the "seams of commodified labor" within the slave economy were wider than Morgan suggests (p. 234n). In what economic terms did freedpeople in central Virginia define the withdrawal of women and children from the work force during Reconstruction? This is an interesting point, one that Morgan might have developed more fully.

Nonetheless, her carefully researched book provides a welcome addition to the new wave of emancipation studies. She reminds historians of the importance of intrastate and intraregional differences both during slavery and in the emancipation process. Virginia's upcountry slaves, for example, offer a strikingly different model than, say, Virginia's tidewater slaves, or slaves in South Carolina's low-country districts, or slaves in Louisiana's parishes. Morgan's book underscores the importance of antebellum ideas of freedom, labor, and property as they were redefined in and by the crucible of emancipation.

JOHN DAVID SMITH
North Carolina State University

R. DOUGLAS HURT. *Agriculture and Slavery in Missouri's Little Dixie*. Columbia: University of Missouri Press. 1992. Pp. xv, 334. \$37.50.

The nature of the antebellum southern economy continues to fascinate scholars. Recently, community studies have captured the southern historiographical limelight. R. Douglas Hurt's book represents yet another contribution to this growing field. Hurt ana-

lyzes seven counties in Missouri's Black Belt, the area he designates as Little Dixie. He uses these counties to provide a window into the economy and society of this often overlooked state of the upper South from its settlement after the War of 1812 until the eve of secession.

Hurt arranges his book topically. He devotes the early chapters to the settlement of the Little Dixie region and to the development of the agricultural economy there. He finds land fever was rampant, land claims were muddled by the French and Spanish legacies, and that the region was aptly suited for staple crop cultivation. The people who settled Little Dixie hailed primarily from Virginia, Tennessee, and Arkansas. Hurt contends that this immigration affected the area's subsequent development: the initial settlers brought with them their southern cultural predispositions. But Hurt finds that the "Missouri River tied [Little Dixie's] economic fortunes to St. Louis" (p. 70). Because of this, the farmers of Little Dixie, although they relied on staple crop cultivation and slave labor, became forward-looking, profit-motivated commercial farmers.

Most of Hurt's work concentrates on the evolution of the tobacco and hemp crops, and the way locals grappled with the chronic problems of specie shortages and poor transportation links (with the exception of the Missouri and Mississippi rivers) to the Deep South and the Midwest. He finds the farmers in Little Dixie were interested in the latest technological innovations and agricultural techniques. He attributes this interest to the farmers' market orientation and their ongoing attention to agricultural improvements. Still, Hurt finds that agricultural societies and fairs, created to perpetuate such policies, usually died because of a lack of interest and financial support. In general, however, Hurt argues that patterns of cultivation and the overall attitudes of the farmers reflect those of their counterparts in the Upper South.

Hurt's discussion of slavery is perhaps the strongest segment of the book. His chapters on slave management and social control, and on the political implications of slavery, are especially good. Hurt devotes no space, however, to the role of the Missouri statehood crisis in the evolution of the Little Dixie region and in the development of proslavery attitudes. This seems curious, especially given the subsequent history of the state. Indeed, Hurt undertakes an extended discussion of Little Dixie—and Missouri's—reaction to the Kansas-Nebraska imbroglio of 1854-57, but he leaves whatever passions that were aroused in Little Dixie to the imagination of the reader.

Hurt also seems to fall victim to some sweeping generalizations. He notes on every topic he surveys how similar Little Dixie is to the other Upper South states. Hurt continually uses some example of a cultivation technique or a reference to the rhythms of rural life to point to how that represented a "cultural transfer from the Upper South to Little Dixie" (p.

199). Unfortunately, these generalizations are usually not sustained by evidence. Rarely does Hurt grapple with the implications of these cultural attitudes for Little Dixie's subsequent political and social history. He needs to flesh out these similarities to fully support his argument that Missouri was little different from Virginia or Tennessee. This seems to be the weakest element in his treatment.

Despite these flaws, Hurt has produced a creditable monograph. He correctly notes that Missouri is too often overlooked in studies on the evolution of the Upper South. He enhances his arguments by including numerous tables and graphs (on crop production, slave holding, and the like) to provide quantitative evidence to sustain the points he raises. Hurt has provided scholars interested in the Upper South with a wonderful springboard for research into other facets of this often neglected area.

MARY A. DECREDICO
United States Naval Academy

VICKI VAUGHN JOHNSON. *The Men and the Vision of the Southern Commercial Conventions, 1845-1871*. Columbia: University of Missouri Press. 1992. Pp. x, 328. \$39.95.

Historians of the nineteenth-century United States grapple perennially with the enigma of slavery in a bourgeois world. Vicki Vaughn Johnson's study of the commercial convention movement provides new insight into the enigmatic slaveholding elite. Between 1845 and 1871, proponents of southern commercial development held sixteen major conventions in various cities of the upper South and the Mississippi Valley. The meetings attracted some 6,400 delegates, including a host of political notables.

One pillar of Johnson's study is a group portrait of about 5,700 of these men. They displayed remarkable similarity in social background: they were disproportionately lawyers and businessmen, southern-born, well-educated, middle-aged, and affluent. This homogeneity assured substantial agreement—but not unanimity—on issues affecting commercial development, the subject that constitutes the second pillar of the book.

From the movement's birth, delegates sought means to maximize profits on exported agricultural commodities, in part by circumventing northern middlemen. Nearly every meeting grappled with ways to garner federal support to improve the South's transportation network of rivers, harbors, and railroads. Unlike similar groups from other regions, however, the southerners predicated their commitment to commercial development on slavery, which held a central place in their "definition of a good society" (p. 90).

Beginning in 1856, the conventions undertook a "proslavery crusade" (p. 132), which culminated in a rabid campaign to reopen the African slave trade. This overture not only appalled northerners but also

alienated border state southerners. It fed directly into the sectional animosity that culminated in secession and the Civil War. In those circumstances, the southern elite had other concerns than commercial conventions.

When the movement reemerged in 1869, the delegates' group portrait looked remarkably like that of their prewar counterparts. Again they sought federal funds for internal improvements and, while they were at it, relief from wartime tax burdens and discriminatory national banking and currency laws. And they argued that prosperity depended on commercial agriculture and a tractable labor force. Nonetheless, by 1871 the movement expired, this time for good.

The only flaw in Johnson's fine book lies in her failure to examine the interclass conflicts generated by the elite's vision of commercial development. The major issues addressed by the convention movement before and after the Civil War—in particular, building railroads and guaranteeing a subservient labor force—had enormous political consequences for European-American laborers and yeoman farmers as well as African-American slaves and freedpeople. As these classes matched their aspirations against those of the elite, a complex internal politics arose that both complemented and complicated the lines of tension between North and South and between lower South and upper South that Johnson traces.

Among other things, appreciating this wider arena of class conflict helps clarify the vexed question of the apparent continuity in background characteristics and "perceptions and beliefs" between antebellum and postbellum delegate groups (p. 192). By delegitimizing slave property, the Civil War forced a reconfiguration of social relations (and, as a result, of politics) within both the South and the nation. Members of the southern elite experienced firsthand what it meant to have slaves no more. Commercial conventions in the antebellum pattern simply did not fit their needs.

Despite this reservation, Johnson's well-written monograph offers a useful addition to the literature on the nineteenth-century South.

JOSEPH P. REIDY
Howard University

ANN PATTON MALONE. *Sweet Chariot: Slave Family and Household Structure in Nineteenth-Century Louisiana*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1992. Pp. xiv, 369. \$39.95.

Applying to the plantation society of antebellum Louisiana the investigative techniques Peter Laslett and his colleagues pioneered for the analysis of family and household structure in pre-industrial England, Ann Patton Malone challenges decades of scholarship on the slave family. Her book is a much-

needed corrective to studies that, in endeavoring to demonstrate the inherent strength of the slave family, minimize the strength of the forces ranged against it.

Malone contends that the strength of "Slave domestic forms" lay in their ability to adapt to external pressures, to bend "like willows in the wind to keep from shattering." Bondsmen and women struggled in a profoundly hostile environment to provide a measure of familial stability for themselves and their children. Their struggles resulted in the development of a range of household types, from the "simple" family, to the female-headed household, to the "solitaire" or one-person unit. Within those three basic household types there were other domestic arrangements designed to meet emotional and material needs, to provide support for all within the slave community, especially the most vulnerable, the old, the very young, the sick or disabled, those bereft of all other kin. As Malone reminds us, during his or her lifetime, an individual slave might live in various types of household settings. And freedom itself could be a mixed blessing, with newly liberated slaves obliged either to jeopardize their hard-won free status by staying with their slave families or break the ties of kinship and strike out on their own.

In this admirable synthesis of the quantitative and the qualitative, Malone advances the discussion of slave life far beyond the notion of a "crop-driven" economy shaping family and community. She argues forcefully against the assumption that plantation life was static over time. Changes at the level of the region and the individual plantation affected slave households in different ways at different times. The trauma of forced separation might well be followed by a period of consolidation, as those left behind sought to adjust to circumstances they could not alter. The ability of individuals to adapt was determined by a multiplicity of factors, from demographic patterns to their sense of self, from a planter's financial stability to his or her notion of what constituted a slave household, from the distances that separated kinfolk to the willingness of planters to permit visits or pass along messages to loved ones.

Malone's prose style is rather dense in places and one can quibble about her choice of Louisiana for this kind of in-depth analysis. Was there anything unusual about the nature of plantation life in Louisiana, or was the selection determined by the availability of archival materials? More attention might have been given to the question of smuggling. As Malone demonstrates, laws were flouted to effect the importation of slaves and "apprentices" from the North. But what of the illegal landing in Louisiana of slaves from the Caribbean and Africa? These are minor issues, however, and, simply put, this book is a remarkable work of scholarship.

Although Malone focuses on one state at one period of time, her findings indicate the need for a thorough reassessment of the contours of family and community life among bondsmen and women

throughout the plantation South. This insightful and finely crafted study is an eloquent plea for historians to avoid oversimplification about a social structure as complex as the slave family. Only through the kind of painstaking work undertaken by Malone can we appreciate the decisions that individual men and women made about their lives, and the degree of freedom they had in reaching those decisions.

JULIE WINCH
University of Massachusetts,
Boston

ROBERT R. DYKSTRA. *Bright Radical Star: Black Freedom and White Supremacy on the Hawkeye Frontier*. Cambridge: Harvard University Press. 1993. Pp. xi, 348. \$47.50.

Robert R. Dykstra's study is one of the most important books ever written about the politics of race in the United States. Dykstra traces Iowa's legislative and judicial actions concerning the status of African Americans from the early territorial days of the 1830s until 1880, when voters removed the last racist law from the state's statute books. In that half century, Iowans wrestled with all the questions connected with slavery and the status of free blacks that convulsed the nation as a whole. These included whether bringing a slave to this nonslave territory rendered the bondsman free (in 1839, the court ruled affirmatively *in re Ralph*); whether to create a "Black Code" imposing restrictions on the rights of free blacks (which Iowans included in their territorial and state constitutions); whether to forbid the migration of additional free blacks to the state (enacted in 1851); if and how to resist the Fugitive Slave Law (which Iowans did, albeit unsuccessfully); and to what extent these restrictions should be eased or abolished after emancipation. Over the period Dykstra examines, Iowa transformed itself from an overtly racist state to one of the nation's most egalitarian, granting blacks full civil and political rights—except legislative eligibility—by 1868. Dykstra attributes this transformation primarily to Iowa's safe Republican majority, which enabled politicians to follow their consciences despite the political risks involved. Iowa's history thus proves that "prejudicial behavior and attitudes can indeed be changed" (p. 269).

One of this study's many strengths is its analysis of the lawmakers' motivations. Iowa's relatively small early population enables Dykstra to become so familiar with the figures involved that he can examine the convictions of the legislators and judges with a detail and precision impossible to attain in a broader study. This is especially important because the racial philosophies of Dykstra's protagonists are often ironically at odds with their actions. Charles Mason, for example, who rendered the *in re Ralph* decision, spent the rest of his career opposing even the most perfunctory civil rights for blacks and became an infamously racist

Copperhead. In another instance, Josiah B. Grinnell, the nationally prominent clergyman, educator, and abolitionist to whom Horace Greeley gave his famous "go west, young man" advice, surprisingly voted against granting suffrage to black Iowans after the Civil War out of fear that it would hurt the Republican Party. Conversely, many avowed racists supported this black suffrage measure. Although Dykstra employs ecological regression statistics to determine what differentiated the "conservatives" from the "progressives," his figures offer few surprises. More insightful is the section of the book that sensibly relates theories of social psychology to his findings.

Dykstra's work is almost entirely a study of politicians and voters. Yet some readers may want to know how racial prejudice manifested itself in venues other than the statehouse or the courtroom. A lynching in early territorial Iowa is described in the opening pages of the book, prompting one to wonder whether racial violence continued to plague the state, and if so whether such violence ebbed as the state's statute books became more egalitarian. One may also question to what extent the lessons learned from the Iowa example can be applied elsewhere. The proportion of Iowans born in free rather than slave states tripled during the 1850s and this, perhaps more than any of the economic or ethnocultural factors analyzed by Dykstra, explains why Iowa could change its stance on racial equality so quickly.

As a study of both racial attitudes and political party development, Dykstra's book is a groundbreaking work, illuminating with uncommon skill and sensitivity the rarely observed intricacies of the process through which Americans removed the legal impediments to racial equality.

TYLER ANBINDER
University of Wyoming

KATHLEEN DIFFLEY. *Where My Heart Is Turning Ever: Civil War Stories and Constitutional Reform, 1861–1876*. Athens: University of Georgia Press. 1992. Pp. xlvii, 236. \$30.00.

Walt Whitman once remarked that the real Civil War did not get into the books, a view that has stood as conventional wisdom among students of Civil War literature at least since Daniel Aaron's *The Unwritten War* (1973). But as Kathleen Diffley playfully observes in this first volume of a projected trilogy based on wartime and Reconstruction periodical literature, the war certainly did get into the magazines. Analyzing 321 pieces of short fiction that appeared in journals ranging from *Harper's Weekly* to the short-lived *Southern Monthly*, Diffley identifies three consistent genres: Old Homestead narratives, Romances, and Adventures. Each genre employed its own version of domestic rhetoric; each carried its own political implications, especially for women and black freedmen. By 1876, Diffley argues, masculinist Adventures—with

their emphases on individual freedom, the open road, and the confinement of women to a strictly private household—came to overshadow Old Homestead narratives, whose older worries about republican preservation had implied a broader, more feminized vision of national citizenship. In between, both chronologically and structurally, were Romances, in which sectional reunion was typically figured as a marriage between a triumphant (and usually masculine) North and a chastened (and usually feminine) South.

Interestingly, Diffley applies her categories not only to magazine fiction but also to congressional debates over adoption of the Thirteenth, Fourteenth, Fifteenth, and Nineteenth amendments. The utterances of representatives and senators are scrutinized for telling domestic metaphors: the merciful mother, the dominating lover, the national household, the sister states, the errant children. By examining congressional debates alongside magazine fiction, Diffley shows that they inhabited the same rhetorical universe and underwent similar evolutions. Along with changes in fiction, she argues, came changes in notions of citizenship, at least insofar as these were expressed in constitutional debates on the floor of Congress: from the "descent" ties of kinship that bound Americans of both sections to the Old Homestead (and the old Union); to the more fluid "romance of reunion" during Reconstruction; to the open road of the Gilded Age, in which citizenship was relocated outside the home as the possession of individual "fellow citizens."

The book includes full texts of three stories, by Mark Twain, John W. De Forest, and Rebecca Harding Davis, which help to anchor the often dense literary analysis that follows. No such aid is available for the other stories treated, and Diffley's readings sometimes plunge ahead without first giving the reader an adequate idea of what these relatively obscure texts say. Social and political historians also may be left wondering whether the connection between domestic fiction and constitutional reform was only rhetorical. Fiction is variously said to have "assisted . . . the growing popularity" of, provided "firm rhetorical ground" for, and "encouraged the figure of" reform (pp. 125, 139, 180), but many other forces were at work on Reconstruction legislators as well.

Still, Diffley has provided a long overdue reassessment of Aaron's amnesia thesis, rescued neglected primary sources from oblivion, and raised some provocative questions about the relationships between gender, race, and citizenship that are approaching the center of nineteenth-century historical inquiry.

STUART MCCONNELL
Pitzer College

DOUGLAS HALE. *The Third Texas Cavalry in the Civil War*. Norman: University of Oklahoma Press. 1993. Pp. xvii, 347. \$28.95.

In this fast-moving contribution to the rejuvenated genre of regimental histories, Douglas Hale provides a chronicle of the battles, marches, and camps of the Third Texas Cavalry, presents "a socioeconomic profile" of its officers and men, and demonstrates how the troopers reacted to "a long war and their ultimate defeat" (pp. xv–xvi). During its four years of service, the Third fought in Indian Territory and Missouri, at Corinth and Vicksburg, and in the disastrous Atlanta and Tennessee campaigns. Although it fought on foot from time to time and was occasionally involved in a pitched battle, its service generally consisted of scouting and picket details, short if sometimes savage skirmishes, and raids on federal lines of communication and supply depots. This is not a romantic account of southern cavaliers; Hale describes the unit's chronic discipline problems, occasionally incompetent officers, and brutal treatment of African-American foes. Yet he sympathizes with these common soldiers and is at his best when describing such hardships as inadequate resources, disease, miserable weather, and dubious decisions by the Confederate high command. He also furnishes statistics on the wealthholding and occupations of the Texans who joined the regiment and briefly explains the postwar careers of a number of the regiment's survivors, arguing that their absence from the war's bloodiest battlefields helped them escape postwar trauma.

Although he makes brief forays into the "new military history," Hale's work is at heart a well-researched narrative written in a clear and often witty style. Hale devotes much of his work to the strategic and tactical contexts of the Texans' service, presenting knowledgeable accounts of most of the major campaigns in the western theater and of the battles in which the Third Texas was involved, however tangentially. As a result, although the regiment was hardly scratched at Corinth, for instance, and provided only a minimal contribution to Confederate operations in the weeks after Iuka, those campaigns still receive detailed attention. Later in the book, readers learn nearly as much about the experiences of the rowdier and less-distinguished Ninth Texas—another unit in General "Sul" Ross's "Texas Brigade"—as they do about the Third.

These problems point out the conceptual tension that exists in any account of a military unit: regiments obviously do not operate individually and the stories of the brigades and corps and armies to which they belong need to be told. Yet authors must also keep in sight the special nature of the unit about which they are purportedly writing. Although chapters are named after and organized around campaigns and battles, the strongest portions of Hale's book deals with the lives and deaths of the soldiers in the Third Texas, but those segments are often eclipsed by long descriptions of campaigns. The Civil War in the West has been dealt with in more detail elsewhere; this useful and energetic book would have been even more useful and innovative if it had stressed the

experiences (Hale's brief discussion of the postwar period is particularly suggestive) of the Third Texas—or even the brigade to which it belonged—far more and larger military issues far less.

JAMES MARTEN
Marquette University

HENRY C. WHYMAN. *The Hedstroms and the Bethel Ship Saga: Methodist Influence on Swedish Religious Life*. Foreword by KENNETH E. ROWE. Carbondale: Southern Illinois University Press. 1992. Pp. xvi, 183.

From 1845 until 1876, the *John Wesley* served as the locus of a remarkable enterprise in the history of Christian missions. Moored at Pier 11 on the North River in New York City and best known as the *Bethel Ship*, the vessel was a floating chapel and headquarters for the ministry of Olof Gustaf Hedstrom (1803–77) to Scandinavian sailors and immigrants. The timing of the venture could not have been better. Hedstrom met the first great wave of Scandinavian sailors and immigrants with tracts, counsel, and invitations to services on board the *Bethel Ship*; in the process, he converted many of them. The origins of Methodism in Sweden and among Swedish Americans in particular can be traced to Hedstrom's mission, as can Methodism among Norwegians and Norwegian Americans. Henry C. Whyman's book is a study of the brothers Olof Hedstrom and Jonas Hedstrom and their influence on Swedish religious life on both sides of the Atlantic.

Whyman focuses on the Hedstrom brothers in seven of the twelve chapters. These are the most valuable chapters, based on data gleaned from scattered sources and uncovering information only vaguely known previously, if at all. The remaining five chapters deal with those who influenced the Hedstroms, notably the Swedish Pietist *Läsare* groups and the pioneer missionary Peter Bergner, or those whom they, in turn, inspired. The latter included the "Swedish Nightingale" Jenny Lind and Victor Witting, who became the father of Swedish Methodism.

Whyman's work significantly advances our knowledge of early Methodism among Scandinavians. His research breaks new ground in several aspects of his subject, notably the life of Bergner, Olof's career prior to his becoming a Methodist missionary, and the career of Jonas Hedstrom. Whyman also writes in an engaging style that makes for pleasant reading; he has an eye for the interesting vignette.

Some wishes one may have for the book remain unfulfilled. Most of Whyman's research is in the contemporary periodical literature and secondary works while relatively little is in primary manuscript sources. Yet these sources do exist. Olof Hedstrom's Norwegian convert, Ole Peter Petersen, for example, wrote at least four manuscripts of reminiscences, and Carl Eltzholtz wrote a biography of Petersen based on primary sources since lost; all contain significant

information on Olof Hedstrom that has escaped Whyman's attention. The author has tried to combine a biography of Olof Hedstrom with a history of early Swedish Methodism but, as a result, the book lacks structural cohesion and is neither. Finally, the book's strength is narrative history, not analysis. Hedstrom was a strong proponent of the doctrine of Christian perfection, for example, but this doctrine, which so differentiates traditional Lutheran and nineteenth-century Methodist anthropology, escapes discussion. Yet it was precisely the doctrine that attracted Ole Peter Petersen, to name but one of Olof Hedstrom's converts.

When all is said, however, Whyman's book marks a significant advance in our knowledge of the Hedstrom brothers in Swedish and Swedish-American life. It is a welcome addition to the history of Swedish immigration, Methodism, and seamen's missions.

ARNE HASSING
Northern Arizona University

ERICH ROBERT PAUL. *Science, Religion, and Mormon Cosmology*. Urbana: University of Illinois Press. 1992. Pp. xi, 272. \$29.95.

For Erich Robert Paul, Mormonism is a good case study in the relationship between science and religion. He argues that "the idea of cosmology is one central organizing theme in Mormonism" (p. ix), and he concludes that Mormonism "has embraced an epistemological, though cautious relationship with modern science" (p. 192). Mormonism, he concludes, for all its suspicion of modern science, managed to escape the "classic error of nineteenth-century Protestant natural theology" (p. 230), namely, the willingness to follow science at the expense of religious convictions. He lauds Mormonism for avoiding that error, a hair's-breadth escape that led by the late twentieth century to an impressive triumph: "Mormonism's emphasis on its metaphysics and eschatology, and not on that part of its epistemology that encourages a realism, protects it from the arrogance of secular humanism." Following David Ehrenfeld (*The Arrogance of Humanism* [1978]), Paul defines secular humanism as a set of assumptions that center on the notion that all problems are soluble, especially by means of science, technology, and social engineering.

In today's rhetoric the triumph could be viewed as a tawdry one, inasmuch as the term "secular humanism" has recently become a right-wing evangelical curseword for intellectual inquiry and higher education. Doubtless the study of Mormon forms of piety would be a kind of cultural history alien to Paul's traditional model of intellectual history: big ideas and a series of figures who embody them. Given this framework, the best part of the book is the trenchant analysis of the career of the brilliant autodidact, Orson Pratt (1811-81). Paul also devotes a long and

interesting chapter to "Extraterrestrial Intelligence [SETI] and Mormon Cosmology," which does get away from biography for a moment, but it tells us more about SETI than about Mormon thought.

Despite these fine aspects of the book, Paul's older model of intellectual history allows little room for nonbiographical aspects of Mormon cosmological culture. For example, it is puzzling that he has given so little attention to that centerpiece of Mormon cosmology—the descent of Enoch's City on Jackson County, Missouri—certainly worth a whole chapter of its own. Another example would be forms of Mormon piety based on the spirit world, like baptism for the dead; if Paul wished to praise the Mormons' continuing emphasis on metaphysics and eschatology and their reliance on divine power, he would have found this and related forms of Mormon piety to be directly related to his theme of cosmology. Even within the biographical approach Paul could certainly have allotted a great deal more thought and space to the teachings and actions of the Prophet himself, Joseph Smith, who, after all, provided the basic paradigm of Mormon cosmology. And the elder statesman of liberal Mormon philosophy, Sterling McMurrin, could have been included among the intellectuals whom Paul interviewed.

In general, Paul's understanding of Mormonism is surprisingly nuanced, even when overshadowed by the history of science. Less friendly writers might have presented such Mormon teachings as the notion that "All spirit is matter" (in Section 131 of the Mormon collection of Smith's revelations, *Doctrine and Covenants*) with a smirk. Paul takes them seriously. Paul's book is a worthy addition to the University of Illinois' impressive Mormon list.

MARIO S. DE PILLIS
University of Massachusetts,
Amherst

MARY P. WINSOR. *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum*. (Science and Its Conceptual Foundations.) Chicago: University of Chicago Press. 1991. Pp. xviii, 324. Cloth \$49.95, paper \$21.95.

Mary P. Winsor's study of Harvard's Museum of Comparative Zoology is much more than an institutional history; it provides a window into the history of systematics (the study of living diversity) and how that method of inquiry fared during the late nineteenth and early twentieth centuries. Although her primary focus is on the museum itself, significant secondary threads include the impact of the theory of evolution on American scientific thought and the maturation of the scientific profession.

Louis Agassiz's dream for the museum, founded in 1859, was that it help people fathom the Mind of the Creator by accumulating specimens for zoologists to identify, arrange, and describe; providing a public

display of the specimens; and training talented young men in the scientific profession. Agassiz kept a close rein on the museum, which had only loose financial and administrative ties to Harvard University. Often he was his own worst enemy, collecting more specimens than his students and limited staff could possibly categorize and occasionally antagonizing those around him. When Agassiz died in 1873 his son Alexander assumed directorship of the museum and agreed to strengthen its relationship with Harvard. The museum, however, never did become a teaching arm of the university. Despite Alexander Agassiz's financial generosity, the institution remained underfunded and thus a basement full of specimens begged for attention as the staff struggled to provide public displays. Concurrently, Harvard professors did no curatorial work and increasingly taught more with the microscope than with specimens.

Much of this shift of zoologists away from the study of specimens came as the result of the publication of Charles Darwin's *Origin of Species* in 1859, the same year that the museum was founded. Winsor's study explores the growing disparity between museum taxonomists, many of whom continued to work in the Linnean tradition, and research scientists who sought to understand the cause of living diversity. In essence, the pressures of daily routine and administration denied museum taxonomists the luxury of time for investigative research. When the specimens in their care were used for scientific research, it was usually by scientists not affiliated with the institution. Stephen Jay Gould has recounted a similar situation: the inability of Charles D. Walcott, head of the Smithsonian Institution, to plumb the significance of his discovery of the Burgess Shale (*Wonderful Life: The Burgess Shale and the Nature of History* [1989]).

Winsor's analysis of the impact of evolutionary thought and the development of the scientific profession is not definitive, but she did not intend that it should be. Rather, she uses these developments to illustrate her point: institutions such as the Museum of Comparative Zoology, caretakers of scientific data, proved unable to assume leadership in investigative research as scientific methodology moved away from the Linnean tradition.

Winsor's volume is insightful, interesting, and readable. Along the way she describes the various personalities who either served or used the museum enlivening what might otherwise have been a dry institutional study with the vagaries of human interaction and exploring the impact of the fast-paced change in the scientific world on them as well as on the museum. Such studies afford us the opportunity to witness, albeit secondhand, the increased specialization of the scientific profession and the evolution of the "tools" designed to serve it.

NANCY SMITH MIDGETTE
Elon College

JOHN PARASCANDOLA. *The Development of American Pharmacology: John J. Abel and the Shaping of a Discipline*. Baltimore: Johns Hopkins University Press. 1992. Pp. xvii, 212. \$32.50.

"Pharmacology" as a word has ancient roots and has possessed varied and flexible meanings. John Parascandola describes in this excellent book the development in the United States of pharmacology as "the experimental science that deals with . . . the action of chemicals on biological systems" (p. xiii). The main focus falls on the half-century from 1890 to 1940, although the first chapter sketches the European background and a short epilogue summarizes pharmacology's great expansion launched by the chemotherapeutic revolution.

John J. Abel deserves, the author says, the appellation "Father of American Pharmacology" (p. xv). A University of Michigan graduate, Abel spent seven years (1884–1891) at European medical schools, receiving his M.D. from Strasbourg where he studied with Oswald Schmiedeberg, who institutionalized the new scientific discipline of pharmacology and whose institute was a mecca for students from all over the world. Abel returned to Michigan, then moved to Johns Hopkins University when its medical school opened in 1893. There he replicated in the United States the pattern of influence of his mentor Schmiedeberg, insisting on the distinctive place and importance of his new discipline, creating a department, instructing a generation destined to be leaders in the field, and playing the principal role in forming a national organization and establishing a journal. Abel was the first president of the American Society for Pharmacy and Experimental Therapeutics, formed in 1908, and he edited for over two decades its journal that began publishing the next year.

Parascandola gives the expansion of pharmacology within American academia detailed treatment. Other leaders besides Abel learned directly from Schmiedeberg, like Torald Sollmann of Western Reserve. With the new century, the pace quickened at reform-minded medical schools, mostly generated by Hopkins-trained professors. The author cites a dozen brief case histories. Graduate training leading to the Ph.D. began in the World War I period; earlier, M.D.s had dominated. Schools of pharmacy and veterinary medicine began to establish pharmacology departments.

Early in the century, the need for pharmacologists grew in government bureaus, especially the Hygienic Laboratory of the Public Health Service and the Bureau of Chemistry of the Department of Agriculture, as these agencies were given responsibility by Congress for the quality of vaccines, drugs, and food. Such regulation was a factor in bringing pharmacologists into the pharmaceutical industry; their presence not only helped standardize old drugs but also accelerated the quest for new medications. Academic pharmacologists long held a bias against their indus-

trial colleagues, who were banned from membership in the professional society until 1941.

Parascandola, using mainly primary sources, has written a model institutional history of a scientific discipline. He specifically disavows intending a complete biography of Abel or giving "significant attention" to "pharmacological methods and theories" (p. xv). The specific matters of research attention, however, of Abel and other academic pharmacologists, as well as governmental and industrial laboratories, receive succinct definition. Portraits of the principal figures and laboratory scenes illustrate the volume.

JAMES HARVEY YOUNG
Emory University

THOMAS R. PEGRAM. *Partisans and Progressives: Private Interest and Public Policy in Illinois, 1870-1922*. Urbana: University of Illinois Press. 1992. Pp. xiv, 297. \$42.50.

Like so many other historical terms and designations, American Progressivism has of late undergone serious complication and modification. If we knew what we were talking about ten years ago, we certainly have less confidence today in the steadiness of periodization and the character of social and intellectual movements. As qualification has piled on qualification, it becomes more difficult to evaluate, let alone answer, the old question, "What was progressive about Progressivism?" Although there are some perils in the contemporary multiplication of perspectives and interpretations, I think, on the whole, this is a very good thing.

One encouraging sign is Thomas R. Pegram's intelligent and interesting book on Illinois Progressivism. Given the prominence of Chicago in the creation and spread of Progressive ideas, this is a fruitful subject. After all, Chicago was the site of Hull House and the base of reformer Jane Addams, arguably one of the two or three most important reformers of the prewar era. The University of Chicago housed some of the most advanced and socially active academics in the nation. Its sociologists, philosophers, and educators were known to all of the advanced thinkers of the day. It was the location, in 1912, of the most optimistic moment of the era, the nomination of Theodore Roosevelt as presidential candidate on the Progressive Party ticket. Its labor unions were aggressive and politically active. In many respects, in institutions, city planning, and literature, Chicago was a leader and innovator. But, Pegram asks, how did this matter? Did this reform potential find its way into policy, particularly the policy of Illinois? The answer, he discovers, is complex, but for the most part, it is also no. The problem was the ambitious but ultimately ineffective attempt of Progressives to define and empower a distinct public interest. By this they meant a civic commitment, above and beyond immediate

political considerations and self-interest, that elevated the common good. Administrative reform of government, regulation of foods and drugs, factory regulation, school and transportation improvements, and oversight could certainly be considered in the public interest. And there were reformers who actively sought deep changes in each area. Some succeeded, but most efforts ended in compromise or even defeat.

Progressives failed for several reasons, but primarily because they misjudged the appeal of their claim to represent the larger public interest. Pegram argues instead that twentieth-century politics was simultaneously undergoing a different transformation, into what he calls "marketplace pluralism." Marked by partisanship and the deep influence of powerful interest groups, politics rewarded those who understood and bewildered those who did not. Thus, the best efforts to reform the schools, secure safe milk, amend the archaic city charter, and improve factory regulation were frustrated.

Just what this says about Progressivism on a large scale is, inevitably, still unclear. Could the movement succeed only on a national scale, or were the same pitfalls of marketplace pluralism operative here too? Were there special circumstances in Illinois that prevented it from following the path of Wisconsin? If "administrative Progressivism" failed, did the reforms proposed by women reformers and social workers fail too? Clearly the answer is beyond the scope of this book, but there are some suggestive beginnings here.

JAMES GILBERT
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College Park

JON C. TEAFORD. *Cities of the Heartland: The Rise and Fall of the Industrial Midwest*. (Midwestern History and Culture.) Bloomington: Indiana University Press. 1993. Pp. xii, 300. \$39.95.

Once again, Jon C. Teaford challenges urban historians to look beyond one city in order to consider wider societal trends. In this study, Teaford explores how, over two centuries, midwestern cities changed "from trading outposts, pinpoints of habitation in the overwhelming wilderness, to industrial giants with billows of smoke testifying to their manufacturing might, and finally to vast conurbations defying description" (p. 255). By taking a regional approach, Teaford identifies both similarities and differences among midwestern cities, as well as ways in which they are distinctive from other regional urban systems.

Teaford traces this collective biography through seven chapters that chronicle the rise of the Midwest. He begins by setting out factors that shaped the urban network in the first half of the nineteenth century: the Ohio and Mississippi river systems; the Great Lakes; state government centers; and the rail-

road. Success initially came from commerce and trade, and determined the contours of the midwestern urban system. Teaford then traces the close connection between the rise of midwestern cities and industrialization in the late nineteenth and early twentieth centuries. Teaford ends his tale on a bleak note, chronicling the onset of the Rust Belt, as midwestern cities were outpaced by those of the Sun Belt.

Teaford dismisses the work of other historians, including David R. Goldfield and Timothy R. Mahoney, whose recent studies also consider regional urban systems. Teaford argues that these historians identified urban regions simply on the basis of trade links, whereas he "considers the economic rise of these communities and their yearnings for cultural success" (p. vii). He devotes two chapters specifically to the cultural achievements of the Midwest, especially those at the turn of the century.

Teaford makes an interesting assertion for a collective culture for the Midwest based on its Teutonic roots. He feels that the significant German populations in nineteenth-century midwestern cities have helped to shape a distinctive regional culture.

Teaford argues that industrialization brought midwestern cities their heyday, and that deindustrialization has led to their relative decline. Interestingly, Teaford does not finally return his discussion to the economic bases that initially shaped the midwestern urban system: commerce, trade, and finance. Whereas many midwestern cities have been devastated by deindustrialization, others remain strong because of their commercial roots. Armed with an understanding of these roots, historians could remind economists, businessmen, and the general public that midwestern cities existed before industrialization and may well have a long history after deindustrialization.

ANN DURKIN KEATING
North Central College

DOUGLAS FLAMMING. *Creating the Modern South: Millhands and Managers in Dalton, Georgia, 1884-1984*. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1992. Pp. xxxi, 433. \$42.50.

Crown Cotton Mills Company was created by local entrepreneurs in the northwest Georgia town of Dalton in 1884. The mills operated until 1969. This book, in the words of Douglas Flammig, "is the story of how mill hands and managers brought the industrial revolution to the American South" (p. xxii). Flammig seeks through a "microanalysis" of this milling community to demonstrate how complex and ambivalent industrial relations can be. He also attempts to show how a localized study such as this can provide insights to the process of modernization in other areas of the South and the nation. To a large degree he succeeds in both goals.

Using the extensive company records, local private and public sources, the census and other government materials, and oral history, Flammig traces the history of the mill community through four distinct periods. The first spans the years from the founding of the firm to World War I, when the work force was drawn largely from the women and children on surrounding farms. The second was the 1920s, when the company's success forced it to look to whole families for labor, which required the building of a mill village that in turn underpinned an effective paternalistic system. Then came the Depression, when the inability of the company to maintain its side of the paternalistic bargain meshed with national trends to result in unionization and the rupture of the "company family." The final period included World War II and the years up to the mill's closing.

This final section is especially valuable, for it is the most thorough examination of the far-reaching changes that occurred in recent decades in the industry and in the lives of its workers. The improved economic conditions for the mill hands that came with unionization and the general prosperity of the war years and the decade following resulted in a rising living standard. The experiences of those who went to war and figuratively "saw Paris," the passage of child labor and compulsory education laws, and a growing variety of employment opportunities throughout the South shattered the ties of the young to the mill. The last vestige of the mill village and the mind-set it represented disappeared with the sale of the houses to the occupants in 1953. About the same time, Crown began to suffer from the dramatic changes in the textile industry world-wide. An attempt to improve profits through reducing labor costs led to bitter conflicts with the union. But nothing seemed to check the losses at the mill, and the directors chose to redirect the company's remaining resources to the manufacture of carpet. The mill closed in 1969.

In the past fifteen years or so, a sizable group of local and regional studies have appeared that examined the process of industrialization in the South. They have demonstrated a wide diversity in both the factors that stimulated industrial development and the societal adjustments that accompanied it. Flammig's excellent study of how the textile industry grew and endured in northwest Georgia and the changes in the lives and attitudes of its people in some ways only adds to the difficulties of generalizing about the industrializing South. He paints a much more complex and ambiguous picture of the relations between worker and management, of the adjustments of a rural community and its people, and of the society that industrialization produced than have most of his predecessors in the field. Yet the vastness of his sources, the breadth of his coverage, and the sophistication of his analysis have enabled him to

provide sound insights toward understanding how industrialization affected the South.

JAMES V. REESE
Stephen F. Austin State University

RICHARD NISWONGER. *Arkansas Democratic Politics, 1896–1920*. Fayetteville: University of Arkansas Press. 1990. Pp. x, 332. \$29.95.

Geographic imbalance has been a chronic problem in the writing of southern history. For more than a century, historians of the American South have focused the lion's share of their attention on Virginia, South Carolina, North Carolina, Georgia, Alabama, and Mississippi. This has been true not only in the monographic literature but also in most works of regional synthesis. Far too often historians have mistaken the "heart of Dixie" for the entire regional corpus. Whether or not this subregional fixation is fading is unclear, but there are signs of hope. A case in point is Richard Niswonger's study. This carefully crafted, unpretentious monograph recounts the political drama of the Progressive Era in Arkansas, one of the South's most neglected and least understood states.

The topic is fresh, but the approach is familiar. Another in the long line of southern "state studies," this book displays both the potential strengths and the common weaknesses of this genre. Niswonger's meticulous research in newspapers and public documents, scrupulous attention to detail, and subtle understanding of the nuances of Arkansas politics have resulted in a richly textured narrative. He has made a valiant effort to separate fact from folklore, not always an easy thing to do in "the land of the razorbacks," and he has avoided hagiography and political partisanship. Consequently, his extended treatments of the state's major political personalities—James K. Jones, Jeff Davis, James P. Clarke, George Washington Donaghey, Joseph T. Robinson, and Charles Hillman Brough—are even-handed and insightful.

Unfortunately, like many state studies, this book suffers from a parochial disregard for the broader contexts of southern politics, southern culture, and national life. This is state history, plain and simple, as the author makes no attempt to place Arkansas's experience in a comparative perspective. This oversight is surprising because there are so many potential points of comparison and connection. Indeed, the basic themes of Niswonger's study should be familiar to anyone who has studied the post-Reconstruction South carefully: the shift from Bourbon Democracy to Bryanism in the mid-1890s; the wrenching and sometimes disillusioning transition from Populism to Progressivism; the restrictions imposed by one-party politics and a colonial economy; the political salience of urban-rural conflict, localism, and intrastate sectionalism; the institutionalization of disfranchisement, bossism, and machine politics; the maturation

of the Jim Crow system and the intensification of white supremacist ideology; and the complex interplay of race, class, and regional consciousness that fostered an emotional, charismatic style of "demagogic" politics. The personal relationships that propelled these developments were the distinctive products of a particular place, but most of the underlying structural realities were variations on a regional theme. Arkansas is different, as V. O. Key noted in 1949, but it is not that different. In any event, what did or did not happen in Arkansas between 1896 and 1920 merits consideration in the ongoing debates about the origins of the New South, the twists and turns in the "Prussian Road," and the myths and realities of Solid South politics.

RAYMOND ARSENAULT
University of South Florida

ELLEN KNIGHT. *Charles Martin Loeffler: A Life Apart in American Music*. (Music in American Life.) Urbana: University of Illinois Press. 1993. Pp. xv, 345. \$42.50.

Charles Martin Loeffler (1861–1935) was one of the outstanding composers of early twentieth-century America. Conductors and performers of the era considered his music at the level of Claude Debussy. He was also a renowned violin and viola virtuoso, similar today to Pinchas Zukerman. Alas, orchestras now never touch his music. Few concertgoers even know of Loeffler.

Previously, the only writing on Loeffler could be found in a few articles and music texts. Ellen Knight's biography is the first on this gifted composer and performer. The work corrects several misconceptions and fills in numerous narrative gaps.

Known as one of the first non-Germans significantly to influence the development of American concert music in the late nineteenth century, Loeffler, Knight clarifies, was actually German-born. He liked to posture Alsatian roots because he so bitterly detested his native Germany, his father having been imprisoned for criticizing Otto von Bismarck and Kaiser Wilhelm, treatment that contributed to his premature death.

Loeffler's political leanings meshed with certain aesthetic precepts that came forth in his music, mainly his attraction to French Impressionist modes. Complementing this style, Knight illustrates, was Loeffler's love of exotic topics: classical literature, Russian peasantry (as a child, he had spent several years in a Ukrainian village), and, later in life, African-American jazz. All this lay at odds with the German-based musical norms of prior generations.

Popular in his day among the Boston Bluebloods among whom he thrived, Loeffler, Knight reveals, did have his detractors. In 1924, for example, the Chicago Symphony considered playing the composer's *Memories of My Childhood: Life in a Russian Village*, but anti-Bolshevik witch hunters protested. Here, as

well as in his love of the music of George Gershwin and Duke Ellington (Loeffler regularly traveled to Harlem to hear the young Ellington perform), Loeffler did not fit the then-politically correct modes of classical music aficionados. Otherwise, as Knight shows, Loeffler was in all outward appearances a perfect citizen of the Boston aristocracy in which he lived most of his adult life; he performed in Boston salons and played first violin for the Boston Symphony. Students of the cultural history of late nineteenth and early twentieth-century Boston will enjoy Knight's biography, for Loeffler's life gives insight to the highest intellectual circles. Although Knight does not discuss it directly, the negative image of this Boston society, and hence guilt by association, may be part of the reason why Loeffler is ignored today.

Exploring Loeffler's links with the various cultures that surrounded his life, Knight deftly presents an overall image of a man who always kept a discreet distance. Delightful, urbane, witty, yet never fully knowable, Loeffler was sought after but never understood. Critics praised most of his performances, even ones Loeffler felt to be sub par. His music lay very much in dissonance with the proprieties of the urbane culture in which he thrived. Perhaps the vicarious flirtations with the sensuous decadence that tinged Loeffler's music was exactly what these elite audiences wanted, although Knight neither explores this nor considers how Loeffler felt about it. She gives the reader hints of how difficult it would be to attempt such an analysis. The man could be terribly distant: he had but a few friends, and he remained engaged to one woman for over twenty years. Throughout, Knight's voice is that of the narrator letting the letters and critics' reviews tell the story. Thankfully, Knight never attempts to peer beneath this narrative in a psychological mode (after all, Sigmund Freud did refuse to analyze Gustav Mahler), but a possible analytical framework with respect to a musician is a musicological one. Alas, actual analyses of Loeffler's music are not present. What Knight calls Loeffler's "life apart in American music" remains unexamined. The reader is drawn to the fact that Loeffler was so distant. The man's music, the only thing that makes Loeffler historically significant, still awaits exploration.

ALAN LEVY
Slippery Rock University

CLAUDIA TATE. *Domestic Allegories of Political Desire: The Black Heroine's Text at the Turn of the Century*. New York: Oxford University Press. 1992. Pp. x, 302. \$35.00.

The nearly lost work of African-American women writers of the last half of the nineteenth century carries powerful but subtle, sophisticated but elemental, and, above all, portentous, messages about the intersections of American culture. Those intersec-

tions hinge on gender, race, and class. This is the literature that Claudia Tate reclaims, and this is the theme she presents. It is a thesis worthy of the careful scholarship Tate provides, for voices of these black women writers reverberate in twentieth-century concerns.

Although a title less forbidding than the one Tate chose might have attracted readers, and although historians especially might have preferred simple endnotes to Tate's system of attributions in her text plus endnotes, the work merits reflection. Much of the writing Tate discusses has either languished in dingy archives or been patronizingly referred to as insignificant or trivial. Tate's interpretations illuminate unsuspected dimensions. Through her explanations, we see a resolute group of talented black women writers finding ways to cope with political problems that involve personal and public boundaries as well as basic social justice. Tate shows us the women's efforts to cope through the means available to them, those to be found in women's domestic situations. Here, in "Domestic Desire as Political Discourse," Tate discovers an "Interpretive Model."

The scope of the problems Tate addresses ranges from slave encounters, as in Harriet A. Jacobs's *Incidents in the Life of a Slave Girl*, to women's involvement in personal and social problems of the Reconstruction and post-Reconstruction eras, as in Anna Julia Cooper's *A Voice from the South*, in Gertrude Mossell's *The Work of the Afro-American Woman*, and in the novels of Frances Ellen Watkins Harper, Pauline E. Hopkins, Katharine D. Tillman, and others, eleven in all. Tate looks at marriage, education, the vote, racial justice, and "Love as a Strategy for Revising Spousal Roles" as matters of prime importance to these writers and their intended readers.

The area of black women's responses has been totally neglected in most traditional accounts. Much less has it been understood that black women writers grasped the full meaning of their own and society's traumas and that they addressed these situations in their writing. The novels turn out to be not simplistic recitations of sentimental stories; rather, they become psychological tools fashioned for survival.

Tate has given us a treasure: a valuable new perspective on Reconstruction and post-Reconstruction histories and historiographies in combination with a sensitive cross-cultural insight into women's and, particularly black women's, relation to that culture. Because of her familiarity with Reconstruction and post-Reconstruction historiography, and because of her sensitive reading of the materials she uses, Tate's book deserves an honored place in historical literature.

FRANCES RICHARDSON KELLER
San Francisco State University

MARY MARTHA THOMAS. *The New Woman in Alabama: Social Reforms and Suffrage, 1890-1920*. Tuscaloosa:

University of Alabama Press. 1992. Pp. viii, 269. \$29.95.

The history of Alabama women has not been illuminated by the recent wave of women's studies scholarship. Now, however, Mary Martha Thomas begins to fill this gap. Challenging the stereotype of the "Southern Lady," Thomas portrays black and white women activists who unapologetically played a public role in Alabama Progressivism between 1890 and 1920. Drawing on newspapers, organizational minutes, and brief biographies of key leaders, she dissects the issues and strategies undertaken by the Alabama's Women's Christian Temperance Union, Federation of Women's Clubs, Federation of Colored Women's Clubs, Equal Suffrage Association, Legislative Council, and League of Women Voters.

The bulk of the text considers the white, middle-class, Birmingham suffragists and their attempts to win the vote. During the 1890s—an era which witnessed the legal disenfranchisement of blacks via poll taxes, literacy tests, and property qualifications—Alabama suffragists drew on white supremacist sympathies. They argued that women of property should have the vote to diminish the strength of the Populists and Republicans. Their efforts brought them a partial victory in the form of the franchise for propertied women on questions about municipal bonds or incurring debts. In later suffrage campaigns, racism played a far smaller role. Thomas asserts that white activists between 1910 and 1915 rallied support by emphasizing the need for tools to implement Progressive programs such as Prohibition and child labor laws. The widening women's support, however, could not crack unyielding opposition to woman suffrage. No Alabama newspapers endorsed it and, in a much-publicized case, one teacher trainee was suspended for her pro-suffrage activity. Within their own ranks, suffragists decided to reject both the flamboyant tactics of the National Woman's Party and the racist tone of Kate Gordon's Louisiana-based organization, the Southern States Woman Suffrage Conference. Alabama women's last effort, from 1916 to 1919, was probably the most disheartening. Activists faced a new anti-suffrage organization. They watched supportive legislators fall away and reject their plea. Even in Washington, D.C., Alabama's representatives and senators would not endorse the woman's vote. Thomas's account of such steady opposition leaves her impressed that Alabama women could make gains during the 1920s in limiting the leasing of convicts to private industry, registering black and white women voters, and running for office.

White clubwomen also initiated programs for social justice. Thomas describes their efforts on behalf of temperance, child labor, reformatories for children, juvenile courts, fair labor conditions for women, and an improved educational system with better salaries for teachers, more attractive schools, and vocational training.

About African-American women we learn far less, but the portraits of Margaret Murry Washington, third wife of Booker T. Washington, and the Tuskegee Woman's Club she founded convey the commitment to social problems, racial pride, and uplift that black members exhibited in their organizations. Black clubs offered night classes, settlement houses, support for black hospitals and schools, reformatories for black children, mother's clubs, and endorsed woman suffrage in 1910, eight years before their white counterparts did.

Thomas provides a clear and concise background for readers unacquainted with national efforts for Progressive reform. She is more generous with detail than analysis, but she opens a dialogue that is certain to enrich historians' understanding of race, feminism, politics, and social reform.

KAREN J. BLAIR
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ELLEN WILEY TODD. *The "New Woman" Revised: Painting and Gender Politics on Fourteenth Street*. Berkeley and Los Angeles: University of California Press. 1993. Pp. xxxiv, 414. \$45.00.

Between the two world wars, four New York City realist painters from a group later called the Fourteenth Street School painted the women they observed shopping, at the movies, working as salesladies and in offices in the Fourteenth Street neighborhood where they rented studios. In her analysis of paintings of women on Fourteenth Street done by Kenneth Hayes Miller, Reginald Marsh, Isabel Bishop, and Raphael Soyer, Ellen Todd Wiley brings together the methodologies of art history, social history, and critical theory. The effort, while imaginative and bold, is not entirely successful.

From the point of view of a historian of women, much of the book is a standard narrative account of images of and attitudes toward women in the 1920s and 1930s, especially the undermining of the symbols of the liberated "flapper" and the turn-of-century "New Woman" by consumption and traditional gender valuations resurgent. Todd's use of critical theory often seems more confusing than enlightening, an opaque means of reaching the conclusions, clear from her sources, that each artist was constrained by his or her background and that, although their representing working-class women as well as middle-class women was a breakthrough, none of them could transcend standard gender definitions to give their women subjects the same positive agency or radical stance as men. The book, however, is overwritten and could have profited from tighter editing.

Yet Todd's historicizing of high art and her connecting it with popular culture and the influences of biography and of place is to be applauded. Todd shows, for example, how Marsh's "voluptuous shopper" drew from the popular stereotype of the movie

siren, to be found on billboards and in fashion magazines and photos of movie stars. Marsh combined these popular images with forms from old masters (as did the other artists Todd examines), an implicitly conservitizing technique. Still, I think Todd underplays what I see as a strong satirical element in Marsh's work. Todd also demonstrates how a radical sensitivity to class issues can inform iconographic analysis. For example, Bishop depicted warm, intimate relationships between workingwomen, but she herself maintained a middle-class positioning and never directly engaged "the socially problematic issues of women's labor" (p. 298), especially with the onset of the Depression.

Using the Fourteenth Street complex of department stores, office buildings, movie theaters, artists' studios, and places where radicals met as a microcosm of society in general is effective: Greenwich Village's hold on the imagination of cultural historians may now have a rival claimant. Both historians and art historians will profit from this book's complex contextualizations; mainstream historians of women, too often focused on politics and economics, might find their vision opening up to the power of cultural structures and visual and iconographic representations through examining this book. The book is profusely illustrated, and the striking color plates of the artists' work are a pleasure to look at.

LOIS W. BANNER

University of Southern California

KEVIN WHITE. *The First Sexual Revolution: The Emergence of Male Heterosexuality in Modern America*. (American Social Experience Series, number 27.) New York: New York University Press. 1993. Pp. xii, 263. Cloth \$40.00, paper \$15.00.

Kevin White poses an important question: "Who was the flapper's boyfriend?" In a wide-ranging analysis, White explores marriage manuals, advice columns, bohemian literature, working-class dance hall culture, and statistical studies to analyze the roots of the New Man to accompany the New Woman of the 1910s and 1920s. He finds that middle-class men underwent a crisis of masculinity at the turn of the century because of the rise of the corporation, the close of the frontier, and the emergence of new women's sex roles. As a result, these men constructed new identities to replace the Victorian dichotomy of the Christian Gentleman and the Underworld Primitive. Along the way, they were influenced by films and advertising, which fostered a sexualized self-concept, marriage experts who demanded that men be more accomplished lovers, physical culture and romance magazines that demanded greater personality from men, and bohemians who experimented with new sexual styles. By the 1920s, these influences flowed into the middle class and a new passionate identity was born.

Unfortunately, a clear picture of the New Man

never firmly emerges. In the chapter on modern heterosexual youth culture, readers learn that behavior changed, but no new male model emerged comparable to the Christian Gentleman. Instead, some men longed for purity, while others used the eroticized youth culture of the day to act out primitive styles learned from the male dance hall culture, bohemian authors, and marital experts. Hence, the middle-class boy could enjoy a greater frequency and variety of sex practices. Yet what were the parameters of this young man? White asserts he was the product of the decline of firm Victorian moralities, and therefore his primitive behavior represented a loss and a pseudo-liberation. At the same time, however, these gratification seekers carried a heavy burden as experts, ads, and films demanded they excel at sexual performance and personality, presumably something they did not "really" want. As a result, many young men were anxious and confused; they worried about potency, homosexuality, and personality.

Part of the confusion results from White's heavy use of prescriptive literature and limited focus on the middle-class young men whom he wants to analyze. One rarely sees what young men of the 1920s thought and did, how they interacted with young women also undergoing change, and how they managed to incorporate lower-class primitive styles of male gratification into a middle-class culture. It is one thing to assert that men raised as Christian Gentlemen were anxious in freedom or that they adopted lower-class styles, but it is another to show how they merged new themes with older individual and class identities. Overall, these young men seem manipulated victims of a consumption and dating system that demanded that they be sexual performers. Much is made of the demands on them, little on how they handled or transformed those demands, and certainly very little on how they, along with young women, sought to create new codes of behavior.

LEWIS A. ERENBERG

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SAUL ROSENZWEIG. *Freud, Jung, and Hall the King-Maker: The Historic Expedition to America (1909)*. St. Louis, Mo.: Rana House or Hogrefe and Huber, Seattle, Wash. 1992. Pp. xi, 477. \$27.50.

Saul Rosenzweig has compiled a fascinating dossier on Sigmund Freud's visit to Clark University in 1909 at the invitation of its president, G. Stanley Hall. Besides offering us a careful and comprehensive narrative of the events, Rosenzweig also includes important primary sources: thirty-one letters between Freud and Hall, and Freud's five lectures, all newly translated and ably annotated by Rosenzweig.

The result is the most thorough study we have of the visit that drew together some of the world's most prominent scientists to commemorate Clark University's twentieth anniversary. Among the speakers at

the convocation were two Nobel Prize-winning physicists and twenty-seven other chemists, biologists, and astronomers. The speakers in the behavioral sciences were especially notable, including—besides Freud—Carl Jung and Franz Boas. Among the participants in the psychology and pedagogy sessions were Adolf Meyer, Ernest Jones, A. A. Brill, Sandor Ferenczi, and William James.

Rosenzweig focuses particularly on the relationships between Freud, Hall, Jung, and James, the most prominent men in the field. Hall, interested in exploring sexuality and psychology, celebrated his Viennese colleague. But as much as Freud appreciated the acclaim, he noticed that “there was a touch of the ‘king-maker’” about Hall, “a pleasure in setting up authorities and then deposing them” (p. 13). Jung, at the time, championed Freud’s ideas; James, however, was skeptical, more interested in psychical research than psychoanalysis as a means of discovering hidden layers of human consciousness.

Because the conference marked the only meeting between James and Freud, who were both house guests of Hall, Rosenzweig focuses extensively on their relationship. Although James wrote later that he hoped Freud and his disciples would “push their ideas to their utmost limits, so that we may learn what they are,” he nevertheless denigrated Freud as “a man obsessed with fixed ideas,” admitting that he was uncomfortable with the “symbolism” of Freud’s dream theories (p. 174).

Rosenzweig calls his historical methodology idiodynamic, by which he means “studying the blending of the biogenetic and cultural milieus in the matrix of the idioverse (the individual world of events), with stress on the creative process” (p. 479). Despite the overbearing jargon of his explanation, the result is often satisfying biography. When, for example, Rosenzweig examines possible reasons for James’s discomfort with Freud, he places this reaction in the context of James’s anxiety over his infatuation with Pauline Goldmark, a Bryn Mawr student he had met while hiking in the Adirondacks. This refreshing view reminds us that we are, after all, considering the lives of complex human beings, not of disembodied intellects.

The discipline of psychology was in its early stages in 1909, and as theorists evolved their ideas, allegiances shifted. As he reflects on the impact of the conference on each of the four men, Rosenzweig explores reasons why both Jung and Hall eventually defected from Freud. This study, the result of fifty years of research in the field, reflects Rosenzweig’s authoritative command of his material. Based on prodigious research, it provides a rich account of a critical moment in the history of psychology.

LINDA SIMON
Harvard University

KENNETH TEITELBAUM. *Schooling for “Good Rebels”:
Socialist Education for Children in the United States,*

1900–1920. Philadelphia: Temple University Press.
1993. Pp. xi, 258. \$44.95.

A key contribution of this history of the socialist Sunday schools in the United States from 1900 to 1920 is that it reveals the emergence of a vibrant radical culture within which alternative schools were but one element of a flourishing, dynamic alternative culture. Kenneth Teitelbaum has meticulously reconstructed the history of these weekend schools for worker education and by doing so has restored not only an important aspect of the history of American socialism but of American education as well.

From the turn of the century until the early 1920s there existed, for varying duration, at least 100 English-speaking socialist Sunday schools in at least twenty states and sixty-four cities across the country that enrolled approximately 10,000 students. These weekend schools were organized at the grass-roots level and aimed at inculcating an appreciation for working-class interests and allegiance to socialist convictions. Teitelbaum highlights the schools in New York City, Rochester, and Milwaukee, which were the most active. He documents the continuous expansion of the schools during the first two decades of the century and the abrupt halt to prospects for a national organization due to the combined effects of official repression and internal ideological struggles on the Left.

This study is at its strongest when grappling with the paradoxical tensions inherent in radical educational theory. First was the problem of providing alternative education to working-class children without threatening support for the public schools that provided real opportunity for advancement for the working class. In the end, public schooling was supported, but its practices, controlled as they were by the capitalists, were criticized. Socialist education in the form of Sunday schools would supplement, not replace, public schooling. Second, if this supplemental alternative education was intended to free the child intellectually, emotionally, and culturally from capitalist domination, it was also aimed at producing “good rebels” inculcated with a class-conscious socialist vision. Pedagogical ideals of critical thinking easily descended into propaganda, such that Teitelbaum concludes that socialist Sunday school teachers “were more concerned with socialist content than instructional process” (p. 198).

The minor weaknesses in this study lie in the compelling questions left unexplored. For example, vocational education, with its particular relevance to the working class, is an issue Teitelbaum alludes to but does not examine. Also, Teitelbaum is perhaps too hasty in claiming that socialist Sunday schools had “no religious character”: American socialism in the early twentieth century was infused with a religious dimension.

This book draws on recent work in educational theory by Michael W. Apple and Henry A. Giroux

that moves beyond social reconstructionist and functionalist interpretations and instead accounts for the contested terrain of American education. Teitelbaum is particularly successful in integrating this educational theory into a Gramscian political framework to understand socialist Sunday schools as a counterhegemonic component in a larger effort to contest a dominant capitalist culture. By illuminating past curricular theory and practice that challenged the dominant ideology, this study challenges the selective tradition that has ignored those critical perspectives and constructive alternative ideas that have a bearing on the culture wars of today.

JOHN A. SALTMARSH
Northeastern University

ERIC B. GORHAM. *National Service, Citizenship, and Political Education*. (SUNY Series in Political Theory: Contemporary Issues.) Albany: State University of New York Press. 1992. Pp. x, 282. Cloth \$49.50, paper \$16.95.

National service represents a persistent strain of utopianism in American political thought. From Edward Bellamy, who authored *Looking Backward* more than a century ago, to William James, who uttered the immortal phrase "moral equivalent of war" earlier this century, to Morris Janowitz, Charles Moskos, and now Bill Clinton, national service has been seen as the answer to any number of social ills. This, in contrast, may be the first critical book-length assessment of "national service," either compulsory or voluntary work for or coordinated by the federal government.

Eric B. Gorham eschews many of the standard criticisms of the program, particularly its costliness and implications for individual liberty, which he acknowledges to be "powerful" (p. 1), and focuses instead on the role of national service as a form of political education. Advocates of national service have long argued that one of its most important purposes was to inculcate citizenship values. There are a host of other goals as well, but, Gorham argues, "without the rhetoric of citizenship one cannot easily find ideological and ethical reasons for youth service" (p. 11). His goal in this book is to demonstrate the program's "fundamental nature: an institutional means by which the state uses political discourse and ideology to reproduce a postindustrial, capitalist economy in the name of good citizenship" (p. 1).

Gorham begins by exploring the arguments of national service proponents. He devotes another chapter to current proposals (written before President Clinton's recent offering). Gorham makes an intriguing distinction between "compulsory" programs, which "provide economic or social sanctions for nonenrollment" (p. 37), and "coercive" programs, which threaten fines or jail for nonparticipation. He also discusses what a national service program might

look like and what kinds of tasks might be undertaken by the young.

But the heart of this book is Gorham's assessment of the program's impact on political and social acculturation. Proponents of national service usually sell their proposals as being nonpolitical, but Gorham rightly points out that participants "also serve the state" and that "this would have immediate political consequences for the party in power" (p. 59).

Moreover, Gorham questions the standard assumption that "service work benefits the moral and intellectual development of the young citizens" (p. 63). To the contrary, he argues, it may prove counterproductive. What these jobs really do, in his view, is provide "training for a future postindustrial economy—an economy that demands an extensive service proletariat" (p. 63).

Although that conclusion seems overstated, Gorham effectively de-glamorizes the concept and the sort of values likely to be learned by participants. Particularly telling is his critique of the Civilian Conservation Corps, "where citizenship education either did not exist, or where it promoted a weak conception of the citizen" (p. 130).

And although the vision of political education that he lays out near the end of this book demonstrates his leftist perspective, this detracts little from the book or his conclusion: "It is easy to be in favor of service . . . The question is, however, in whose service national service will be employed, and whether such services enrich the moral life of every citizen" (p. 209). The answer, Gorham rightly suggests, is not what proponents of national service would have us believe.

DOUG BADOW
Cato Institute

THOMAS J. JABLONSKY. *Pride in the Jungle: Community and Everyday Life in Back of the Yards Chicago*. (Creating the North American Landscape.) Baltimore: Johns Hopkins University Press. 1993. Pp. xviii, 190. \$38.50.

What more remains to be said about the "Back of the Yards" area of Chicago, first made famous by Upton Sinclair's novel *The Jungle*? James R. Barrett described how labor militancy and class solidarity allowed workers and their families to forge structures of community to counter the oppressive world of the meat packing plant and stockyard early in this century (*Work and Community in the Jungle* [1987]). Similarly, Robert A. Slayton explored how local democracy emerged out of family life, church parishes, politics, and, finally, the Back of the Yards Neighborhood Council organized by Saul Alinsky and Joseph Meegan in 1939 (*Back of the Yards* [1986]). Now, in this brief volume, Thomas J. Jablonsky uses the methodology of ethnography and historical geography to trace the area's transformation from notorious slum to closely knit neighborhood community during the

interwar years. His background clearly qualifies him for the task: he was born in the area and lists his parents among the eighty-four individuals whose oral history interviews form a crucial source for this study.

The first third of the book introduces the grim geography of Back of the Yards prior to 1918. Cut off from surrounding neighborhoods by packing plants, the infamous Bubbly Creek, railroad tracks, and Garfield Boulevard, residents developed a sense of boundedness and isolation. This was the "Jungle" of high job turnover and ethnic succession in which new immigrant Poles, Bohemians, and Lithuanians constructed tightly knit communities around ethnic Catholic parishes. Yet the diversification of employment opportunities, immigration restriction, and the assimilation of older ethnics in the 1920s altered the socio-spatial orientation of residents. Where family and household had once formed the center of their nonwork lives, now these competed with nonfamily agencies such as schools, parks, and shopping strips. Back of the Yard residents were able to create a "better-developed, more inclusive street life" (p. 101), while the spanning of ethnic boundaries created a sense of place and an awareness of neighborhood identity that culminated in the formation of the Neighborhood Council.

Jablonsky's book is ambitious in its effort to probe the spatial awareness of residents and link it to the evolution of the local community. But his effort to date the emergence of such an awareness is unconvincing. His discussion of spatial context drawn from Progressive sources is only loosely linked with material gleaned from his interviews with neighborhood residents focusing on the Depression. The numerous photographs of the Back of the Yards that provide a striking visual context for the neighborhood mainly fall outside the period 1918–41, the author's principal period of focus. And his geographic analysis of ethnic residence is singularly unrevealing. The aggregated census data drawn at the tract level lacks the detail necessary for the highly localized analysis that he wishes to undertake. Even his fascinating ethnographic analysis of material culture, interethnic conflict, and social life seems insufficiently theorized and often lapses toward nostalgia. Compared with other recent scholarship on Chicago by Lizabeth A. Cohen (*Making a New Deal* [1990]) and Slayton, Jablonsky's book, while useful, is nonetheless bound to disappoint.

KENNETH A. SCHERZER
Middle Tennessee State University

RICHARD W. THOMAS. *Life for Us Is What We Make It: Building Black Community in Detroit, 1915–1945*. (Blacks in the Diaspora.) Bloomington: Indiana University Press. 1992. Pp. xiv, 365. \$47.50.

This book follows a well-traveled trail in black urban history. After 1915, labor shortages sparked a mass

migration out of the South. Black men joined Detroit's rapidly expanding industrial economy, while black women entered domestic and personal service. The Detroit Urban League linked black and corporate Detroit, handling job placement, offering social services, and teaching newcomers middle-class values and urban survival skills. Migrants endured poor housing and poor health and suffered racial bias in housing, employment, and public accommodations. Meanwhile, Motor City blacks built community institutions and protested injustice. In the 1930s, traditions crumbled when Democrats eclipsed Republicans and when the United Auto Workers (UAW) broke Ford Motor Company's hold on the black ghetto.

As a survey of black Detroit from World War I through World War II, Richard W. Thomas's book serves as a sequel to David Katzman's *Before the Ghetto* (1973). The virtues of Thomas's study lie in its comprehensiveness and its coverage of local black institutions and leaders. Thomas also briefly suggests the southern roots of migrant behavior in the big city, although this theme deserves even more attention. When Thomas deals with topics previously explored by others, however (such as the Sweet case, the rise of UAW, and wartime racial violence), he breaks little new ground.

This monograph has two main difficulties. First, Thomas uses a "community building" model that he claims is "a more holistic perspective" than "ghetto formation" or "proletarianization" approaches to black urban history. Actually, his study is less original than Thomas suggests, for its structure and arguments follow previous work in the field. Moreover, Thomas uses "community building" in such an all-encompassing way that specific meaning is elusive. Any hardship, general trend, or specific action advances "the community building process." For example, migrants had a hard life, but adversity "welded them into a community" (p. 88). Blacks' ties to Ford helped the community-building process, but so did the rise of the UAW. Surely black Detroiters formed a community, but the concept of community requires more probing treatment than it receives here. For example, Thomas abundantly documents important differences among blacks (based on class, gender, politics, length of residence in Detroit, and place of birth [United States or West Indies]), but he fails to reconcile these divisions with his repeated refrains about "community building." In fact, he downplays the significance of class rifts in the ghetto (pp. 247–48, 264), a judgment his evidence disputes.

The second problem is the need for skilled editing. Frequent long quotations, flawed paragraphs (some are single sentences, others are disjointed), clichés ("handwriting on the wall" [pp. 27, 202]), and erroneous and contradictory information distract the reader. Some statements are wrong (most black southern migrants "joined the ranks of the middle class" [p. 16]); others are highly judgmental (black voters "followed like sheep" [p. 259]); still others are

puzzling (the NAACP's *Gloster Current* was a "radical" [p. 234]). Twice the same phrase or episode appears in separate locations in the text (pp. 244 and 297, 286 and 293). Chapter conclusions do not analyze or add new insights but simply summarize. The important section on political change is marred by non-chronological coverage. Accordingly, those interested in black Detroit will welcome this book, but its appeal to a wider audience may well be limited.

MICHAEL W. HOMEL
Eastern Michigan University

EMILIO ZAMORA. *The World of the Mexican Worker in Texas*. (Centennial Series of the Association of Former Students, Texas A&M University, number 44.) College Station: Texas A&M University Press. 1993. Pp. xii, 285. \$39.50.

This is an important study about how workers of Mexican extraction in south Texas organized to combat low wages, social discrimination, poverty, and the exclusionary practices of the American Federation of Labor (AFL) during the first two decades of the twentieth century. Emilio Zamora uses archives in Mexico and the United States to challenge older labor histories that depicted Mexican workers as sluggish and unorganizable. Employing a New Labor history approach, he examines the organizing efforts of Mexican workers in the context of their social, cultural, and political experiences in south Texas. His book provides a refreshing look at the blend of cultural traditions and social reality that shaped Mexican workers' collective response to the commercial transformation of south Texas around the turn of the century. It also sheds light on how American economic penetration of Mexico and the politics of the Mexican Revolution influenced the interactions of workers on both sides of the border.

Zamora is sensitive to gender divisions and recognizes that the Mexican community was not monolithic in its cultural identity or political outlook, but he stresses that a nationalist sense of community and political culture facilitated the growth of working-class organizations. He devotes a chapter to Laredo's Federal Labor Union No. 11953 from 1905 to 1907. This socialist union, loosely affiliated with the AFL and composed mostly of Mexican workers, promoted the incorporation of women in keeping with its strong commitment to working-class unity. Union leaders viewed their activities in the broader context of hemispheric working-class struggles. They found Laredo's political climate and binational orientation conducive to unionization, although they suffered a crushing defeat in 1907 when the Mexican National Railways moved its shops to Nuevo Laredo.

Zamora shows that the Partido Liberal Mexicano (PLM), which promoted the overthrow of dictator Porfirio Díaz, energized the Mexican community in Texas, encouraging many to embrace more radical

doctrines. This was especially true after 1910, when the PLM openly criticized the marginalization and exploitation of Mexican workers in the United States. Government repression and anti-Mexican attitudes within the Texas Socialist Party limited the expression of this socialist unionism.

By the 1910s, Samuel Gompers viewed Texas as crucial to the AFL's efforts to forge an alliance with organized labor in Mexico in order to negotiate immigration restrictions. The formalization of ties between the AFL and the Confederación Regional Obrera Mexicana at the Pan American Federation of Labor Conference in 1918 produced an AFL strategy of selective incorporation of Mexican workers. Gompers opposed further immigration but promoted the organization of U.S.-born and naturalized citizens of Mexican extraction. Zamora gives considerable attention to the activities of Clemente Idar, an AFL organizer and labor diplomat who battled deeply ingrained racist attitudes in the Texas State Federation of Labor to implement this strategy.

This book deserves a wide reading. It integrates Chicano, labor, and Texas history, and it contributes to a growing literature on the intersection of the histories of Mexico and the United States.

GREGG ANDREWS
Southwest Texas State University

RENQIU YU. *To Save China, To Save Ourselves: The Chinese Hand Laundry Alliance of New York*. (Asian American History and Culture.) Philadelphia: Temple University Press. 1992. Pp. x, 253. \$39.95.

Up until the 1950s, the hand laundry business was one of the few professions open to the Chinese in the United States. It entailed minimal contact with whites, and therefore the least possibility of racial confrontation. Most hand laundry operators toiled sixteen hours a day, six days a week. They lived where they did the ironing: behind the drab storefronts below the street level. They had no right to citizenship and could not bring their families from China to settle down.

Renqiu Yu tells how in the 1930s New York's Chinese laundrymen, in defiance of isolation and rejection by the American society, formed the Chinese Hand Laundry Alliance to fight attacks from white competitors. Whites had caused the passage of a law in New York City singling out the Chinese laundries for heavy taxation in order to force them out of business. With the help of progressive American lawyers, the order was rescinded. After that the Alliance's role expanded to meet a multitude of the laundrymen's needs. It became the democratic opposition to the corrupt traditional associations that were ruling the Chinese community. It also became the mobilizing force behind patriotic activities in support of China's war effort against Japanese aggression.

The accounts of the patriotic activities of the Chi-

nese laundrymen and the history of the Chinese Hand Laundry Alliance, from its rise in the 1930s to its decline during the anticommunist hysteria of the 1950s, have already been well documented in Leong Gor Yun's *Chinatown Inside Out* (1936), in my own *Chinatown, New York: Labor and Politics, 1930-1950* (1979), and in a number of Him Mark Lai's works, especially his articles in *Counterpoint: Perspectives on Asian America* (1976). In the introduction to his book, Yu declares that he intends to "present the Chinese laundrymen's own perspectives and to place them at center stage" (p. 5). Yet his presentation follows closely the formal positions of the Alliance at all times. His uncritical account reads like the "official biography" of the organization.

Yu announces that he wants to deal with "solid details" (p. 5) about the Chinese laundrymen rather than with historical background. His book indeed contains details. Unfortunately, more quotes from the same Chinese newspaper sources and more oral histories do not make a better book. Interviews with the old-time activists, perhaps due either to their failing memories or their reluctance to revisit the painful past, have yielded no new insights.

By focusing on the Alliance's activities, Yu brings out the fine human spirit of the laundrymen, who fought for survival and self-esteem. But their activism arose in the context of a turbulent time. The intensity of conflicts between the Chinese and American laundries can only be understood properly in the setting of the Great Depression. Chinese laundrymen fought against the oppression and exploitation of the Chinese traditional elite at a time of the rising class struggle in America, which took the form of industrial labor organizing. Moreover, the Chinese could not help but be partisan in the international struggles between communism, capitalism, and fascism, leading to World War II and eventually to the Cold War. During the 1950s, Chinese laundrymen suffered as a result of the intense hostility between China and the United States, which manifested itself in the McCarthy era.

To characterize the laundrymen's activities narrowly as a desire to save China is to write a nationalistic cultural history. Without a proper context to orient readers, the sheer details Yu has collected in his book only amount to a cumbersome reading.

PETER KWONG
Hunter College,
City University of New York

PHYLLIS A. ZIMMERMAN. *The Neck of the Bottle: George W. Goethals and the Reorganization of the U.S. Army Supply System, 1917-1918*. (Texas A&M University Military History Series, number 27.) College Station: Texas A&M University Press. 1992. Pp. 201. \$39.50.

This story by Phyllis A. Zimmerman is about how General George W. Goethals, who had built his

reputation directing the completion of the Panama Canal, tried and failed to rationalize the U.S. Army's World War I logistic system. Shortly after the United States entered the war, Goethals was brought out of retirement, put at the head of an Emergency Fleet Corporation (EFC), and given the task of building a vast armada of ships—wooden ships—that would ferry supplies and troops to Europe. Goethals had misgivings about the project but as a good soldier he undertook it anyway. The EFC entirely justified his doubts, leaving the reinforcement of American troops largely dependent on Allied vessels.

After a few unpleasant months Goethals resigned from the EFC and shortly took on the even more demanding job of reforming the system of procuring, storing, and shipping supplies for the American Expeditionary Force (AEF). He was to do this as acting Quartermaster General and as director of a series of purchase, storage, and traffic bureaus within the war department.

Given the obstacles he had to overcome, it is remarkable that Goethals accomplished anything. He never received clear-cut authority from the Woodrow Wilson administration. He reported to more than one superior at a time. He faced rapidly escalating demands from AEF commander, General John J. Pershing, for supplies that could not possibly be delivered by Pershing's deadlines. He had to work with civilian agencies to make army logistics, organized by military function, fit a civilian production system organized largely by commodities. Most important, he had to gain control of the army bureaus that had longstanding ties to Congress and to suppliers. These bureaus ordered military items in uncoordinated ways with disastrous results, yet they had sole legal authority to purchase and resented attempts by others to tell them how to operate.

Much of Zimmerman's book tells of Goethals's largely futile efforts to coordinate the bureaus and of his unrealized plans to take away powers they misused. Except for a brief introduction and conclusion, this is a fact-after-fact account that pays slight attention to larger issues. For instance, the author tells us that a priorities board disapproved construction of a concrete bridge over Pennepack Creek in Philadelphia. In the next paragraph she gives about equal space to note, just in passing, that priorities were regulated by the industries they were supposed to control, a crucial matter to anyone who wants to understand the American political economy in World War I (p. 110). She does not explain how the Wilson administration's failure to rationalize the war department enabled the army to serve as a check on the power of business leaders who otherwise dominated economic mobilization. She overstates the difference between her own conclusions about Goethals's failure and the conclusions of other historians of war mobilization.

Zimmerman would have benefited from more ru-

mination on the immense subject of which she has described a part.

RONALD SCHAFER
California State University,
Northridge

DOUGLAS B. CRAIG. *After Wilson: The Struggle for the Democratic Party, 1920–1934*. Chapel Hill: University of North Carolina Press. 1992. Pp. viii, 405. \$45.00.

Douglas B. Craig's study, an important reexamination of the Democratic Party from the collapse of Wilsonianism into the era of the New Deal, raises complex questions of how economic issues interact with cultural politics. Conflict within the party, he agrees, was between cultural forces that historians have already defined: roughly speaking, a constellation of urban, ethnic, antiprohibitionist voters of the East, many of them Roman Catholic, and old-stock, dry, rural and small-town Protestants of the South and West. Craig now adds, with an impressive mass of well-arranged evidence, that the clash was also between the conservatism of the political leaders of the Northeast and the Progressivism of much of the hinterland. It is an ideological quarrel that historians have noticed before but have not put at the center of intraparty politics as Craig does. In 1928, Al Smith's friend John J. Rascob became chairman of the Democratic National Committee, and the next year Jouett Shouse was put at the head of the party's publicity bureau. Such men, Craig avers, were responsible for imposing on the presidential wing of the party, until Franklin Roosevelt wrested it from them, a politics of minimal government that was permissive toward big business.

The issue that Craig identifies as central to the strategy of conservative Democrats was the repeal of Prohibition. The Eighteenth Amendment, favored by the party's inland Progressives, represented for conservative Democrats an unwarranted extension of governmental power that threatened by example to sanction further interference in the economy. Legal alcohol would provide a source of tax revenue alternative to the incomes of the wealthy; the Depression added to the case for repeal the consideration that in putting people to work the liquor industry would stave off the need for federal relief legislation hated by business conservatives. Craig finds that, in supporters and in philosophy, a straight line runs from the Association Against the Prohibition Amendment through the Repeal Associates and into the Liberty League. He insists that all this was not merely a matter of economic selfishness: antiprohibition conservatives meant their ideology and could articulate it.

Concentrating on the single battle over economic doctrine, Craig leaves to other historians or to a later time the work of discovering the exact ideological connections, or absence of them, between the party's conservative northeastern leaders and the cultural

base of the democracy in that region. Craig has clearly demonstrated the economic component of the repeal movement. But what about resistance to Prohibition as interference not with the marketplace but with a private pleasure? Did affluent Democrats see the Eighteenth Amendment as insolent meddling by western bluenose evangelicalism into the luxuries to which their affluence entitled them? Progressives (an obvious exception is Thomas Walsh, the Montana Catholic dry) doubtless thought of the Roman Catholic church as reactionary; but did northeastern conservatives really think of it approvingly as a defender of status and wealth? Urban ethnics, among them the New York street partisans of Al Smith who, in an enactment of the cultural politics of the day, jeered William Gibbs McAdoo at the Democratic convention of 1924, at the same time constituted at least indirectly the popular source of the power of the party's northeastern conservative leadership. They were also to become integral to the New Deal politics of the following years, which Craig convincingly presents as having its stronger origins in hinterland progressivism.

Craig has significantly altered the discussion of the politics of the Prohibition era. In doing so he has added to the tasks that scholarship must undertake.

DAVID BURNER
State University of New York,
Stony Brook

IAN JARVIE. *Hollywood's Overseas Campaign: The North Atlantic Movie Trade, 1920–1950*. (Cambridge Studies in the History of Mass Communications.) New York: Cambridge University Press. 1992. Pp. xvii, 473. \$65.00.

Although overshadowed by the glamour of making movies, the distribution and exhibition end of the business is at least as important. Export revenues loomed particularly large in the heyday of the Hollywood studio system, roughly 1920 to 1950. In a blend of business and film history, Ian Jarvie analyzes how American pictures came to dominate the North Atlantic movie trade, or, more precisely, the British and Canadian markets since other North Atlantic countries are scarcely mentioned. Following Rachel Low (*History of the British Film, 1918–1929* [1971]), Jarvie argues that after World War I Hollywood filmmakers used the big domestic market to underwrite the high costs of glossy, spectacular pictures. They then turned aggressively to the export market, of which Great Britain was the largest, to make their profits. Limited by their smaller domestic market, British moviemakers could rarely equal American production values.

The inferior position of British films triggered considerable anxiety from both the Left and the Right about Hollywood's presumed malign influence on British culture. Jarvie details the struggles over

quotas, blocked sterling, and other measures that were designed (often illogically) to preserve the British film industry. In the end British audiences preferred Hollywood films, just as American audiences rarely accepted British products. In a striking inversion of the usual arguments Jarvie persuasively contends that "British films owed their existence to, indeed were in a certain way parasitic on, the exhibition industry created around the American product" (p. 172).

Jarvie's book is an indispensable source on the movie exhibition business, thoroughly researched in American, British, and Canadian government sources and some Hollywood industry archives. The wealth of information he commands makes it regrettable that he employs a very awkward organization. Rather than laying out an overall analysis, Jarvie offers what are three largely separate studies. The first 100 pages on Canada should be published in a different format, with only a chapter devoted to it here; the small Canadian market was treated by American exhibitors as a branch of the domestic industry. The British and American sections need to be interwoven instead of treated as parallel but distinct cases, which makes chronology and relationships between the two confusing. The book would profit from condensation. Readers willing to make their own connections, however, will find a good deal of valuable information on the mundane business that undergirded Hollywood's global glamour.

CLAYTON R. KOPPEL
Oberlin College

ROBERT MOATS MILLER. *Bishop G. Bromley Oxnam: Paladin of Liberal Protestantism*. Nashville: Abingdon. 1990. Pp. 624. \$29.95.

In this excellent biography, Robert Moats Miller characterizes the Methodist Bishop G. Bromley Oxnam (1891–1963) as the "paladin" of liberal Protestantism. As such, the life history of Bishop Oxnam provides important insights not only into the strengths and weaknesses of American Protestant liberalism but also the waning of mainstream Protestant hegemony.

Miller traces how in the years preceding Oxnam's election as bishop in 1936 he had established a well-earned reputation as an advocate of social justice and defender of the rights of labor. Perceived by his conservative opponents, both within the church and society at large, as a radical, Oxnam was in fact a moderate liberal who preferred gradual social reform and the maintenance of law and order to revolutionary change and any fundamental questioning of the status quo. At the same time, Oxnam's experience in rural and urban ministry, especially his founding of the Church of All Nations in Los Angeles as an embodiment of Social Gospel ideals that saw the church as a crucial institution in the service of the

community, provided him with tools of social analysis that made him attentive to the conflicts between labor and capital and the gulf between wealth and poverty. In addition, Oxnam wedded his belief on the role of the church in the public arena with an internationalist outlook on world affairs. His brief experience as a personal secretary to Sherwood Eddy was the beginning of a lifelong interest in international politics and the global nature of the Christian church, particularly within the context of the United States as a world power.

Oxnam as bishop was the quintessential Methodist, both in terms of his bureaucratic skills and a well-scheduled life, who maintained a belief in the institutional church as an effective instrument for societal change. Miller argues that Oxnam was the consummate church bureaucrat who was able, through the force of his own personality and organizational gifts, to further the work of the Federal Council of Churches and the National Council of Churches and helped to lay the groundwork that established both the United Nations and the World Council of Churches.

According to Miller's analysis, Bishop Oxnam is perhaps best remembered for his critique of the repressive methods used by the House Committee on Un-American Activities at the height of the anticommunist hysteria of the 1950s. And yet Oxnam, like many Protestant liberals, was intoxicated with serving the powerful at the expense of prophetic witness. He gave his own stamp of moral and political legitimacy to liberal anticommunism, often unaware of his own capacity for self-deception. Miller deals in great depth with Oxnam's ongoing relationship with John Foster Dulles and his uncritical acceptance of U.S. postwar foreign policy.

Beyond insights into the contradictory world of Protestant liberalism, Miller's biography provides an illuminating perspective on people like Oxnam who assumed that the United States was a Protestant nation and any threat to Protestant hegemony was simply un-American. Miller has thus written a moving portrait of an intriguing figure in the history of American Protestantism. The only major limitation to this otherwise admirable study, one which Miller himself acknowledges, is that there are no footnotes and therefore it is difficult to make use of Miller's extensive research.

ROBERT H. CRAIG
Mount Union College

SARA ALPERN *et al.*, editors. *The Challenge of Feminist Biography: Writing the Lives of Modern American Women*. (Women in American History.) Urbana: University of Illinois Press. 1992. Pp. 210. Cloth \$44.95, paper \$14.95.

The last decade or two has seen a renewed interest in biography among the reading public and within the

academic community. The field of women's studies has contributed to this enthusiasm, and indeed, as the editors of this anthology point out, nearly two hundred biographies of women have been written since 1970. More recently, women writing about the process of writing about women has become a genre all its own, producing such well-received works as those by Carolyn G. Heilbrun, *Writing a Woman's Life* (1988); Carol Ascher *et al.*, *Between Women: Biographers, Novelists, Critics, Teachers and Artists Write about Their Work on Women* (1984); The Personal Narrative Group, *Interpreting Women's Lives: Feminist Theory and Personal Narratives* (1989); and Teresa Iles, *All Sides of the Subject: Women and Biography* (1992). All of these studies bring a new dimension to the art of writing biography. For whereas feminist biographers see their work as an attempt to define the meaning of women's lives by interpreting the life of their subject, writing about the process of writing moves the writer to a new level of consciousness, where the relationship between herself and her subject becomes primary. This development has led to exciting reinterpretations of the lives of both well-known and lesser-known women, but it contains within it the possibility of overindulgence in the personal and the present at the expense of a subject who had a life that was quite separate from that of her biographer. Fortunately, the contributors to this volume are well aware of this potential danger, and although each writer confronts it directly, each seems to be successful in moving beyond "intersubjectivity" to the appropriate detachment of a historian.

The book is a collection of ten essays, each of which includes a short biographical sketch of the subject, which relates her life to the larger issues in women's history. The subjects are all twentieth-century American women: reformers Florence Kelley, Molly Dewson, and Jesse Daniel Ames; politicians Belle Moskowitz and Helen Gahagan Douglas; radicals Emma Goldman and Mary Heaton Vorse; educator Lucy Sprague Mitchell; writer and salon hostess Mabel Dodge Luhan; and editor of *The Nation*, Freda Kirchwey. But the essays are more than biographical sketches. Each one is also a self-conscious attempt to understand the connections between the lives of these women and the personal, political, or ideological issues of the last ten to fifteen years and their impact on the biographers.

The biographers have all spent at least a decade with their subjects: thus their feelings about these women and about the task of writing biography have evolved in the context of great personal and political change. For instance, several felt that in the early stages of their work they had to justify writing about an individual woman—a "great woman," or elitist, approach to history (although, in fact, none of these women are really well known)—at a time when the profession was dominated by the social history approach. Also, each writer had to come to terms with how she felt about her subject as a person and a

historical actor. Not all of these women were feminists; all of the authors are. Not all of the subjects were particularly likeable, and nearly all of them had some obvious failing or weakness. In addition, the authors had to recognize their own biases and how this affected their acceptance and their critique of their subject.

Thus, Alice Wexler confesses that she wanted to like Emma Goldman more than she did, and that she fell out of sympathy with Goldman because of her attacks on the Soviet Union in the 1920s and 1930s. But the events of post-1989 and the possibilities of new scholarship on the Soviet Union has caused Wexler to rethink Goldman's critique. Joyce Antler, in her essay on Lucy Sprague Mitchell, wrestles with the issues raised by women's attempt to combine fulfilling careers with marriage and motherhood. In particular, she points out that professional women are often judged harshly by their children as mothers, and she questions whether it is possible for biographers of women to judge the quality of parenting. Several writers discuss their search for sources and their strategies for filling the gaps in their subjects' lives. Elisabeth Israels Perry describes her painstaking search for primary material on the life of her grandmother, Belle Moskowitz; and Sara Alpern describes her preparation for interviews, as well as the varied nature of her written sources. All of these essays offer interesting insights to the art of writing biography, and from the perspective of the historian rather than the literary biographer, as is more often the case.

Frequently an anthology is characterized by the uneven quality of its contributions, but in this particular collection the quality is surprisingly even. All of the essays are interesting, informative, and quite readable. This is must reading for the historical biographer, as well as the reader of biography.

WINIFRED D. WANDERSEE
Hartwick College

HARRIET HYMAN ALONSO. *Peace as a Women's Issue: A History of the U.S. Movement for World Peace and Women's Rights*. (Syracuse Studies on Peace and Conflict Resolution.) Syracuse, N.Y.: Syracuse University Press. 1993. Pp. xix, 340. Cloth \$39.95, paper \$17.95.

Harriet Hyman Alonso surveys the structures and strategies, individuals and ideologies of female pacifism in America from its origins in the mid-nineteenth century to the present. As the subtitle of her book indicates, Alonso concentrates on those women whose pacifist principles coincided with feminist sensibilities or at least were expressed within the organizational boundaries of associations whose origins stressed women's rights. She does not explore the activism of women who worked for peace along with men in Quaker or other religious groups or within male-dominated societies.

Alonso argues that women's opposition to war and

violence experienced the ebbs and flows of any century-long movement, but that specific themes persisted: insightful connections made among war, domestic violence, and patriarchal power; preference for gender-separate organization; gender-specific notions and rhetoric emphasizing women's special responsibilities as mothers and as citizens.

These themes do not always emerge adequately from the evidence. The radical articulation of links between male dominance at home and in military affairs, and the subsequent cost to women, resonates more clearly in the words of abolitionist feminists and on the contemporary scene than in the decades between. Awareness of the benefits of same-sex activism and resentment over male encroachment is illustrated by an explicit example in the introduction (p. 13) but rarely explored or analyzed again. (Estelle Freedman's evocative article, "Separatism as Strategy" [1979], is absent from the bibliography.) And while Alonso is correct to limit the scope of her study, the achievements of her subjects would be more balanced if they were occasionally placed in broader comparative context. Surely they worked with other pacifists to lobby for the Nye Committee hearings in the 1930s and to organize massive antiwar demonstrations four decades later.

Alonso has, however, made a significant contribution to the literature on an admittedly minority movement. She is especially enlightening in describing the changing demographics of members in important locals, especially the New York chapter of the Women's International League for Peace and Freedom (WILPF), the organization with the longest continuous history and the one on which she concentrates. WILPF provides Alonso with telling examples of the tensions created when national leadership and local membership diverge in terms of social profile, generational experience, and ideology. These clashes mark most organizational experience but are often slighted in histories of national associations.

Equally illuminating, although discouraging, were the internal conflicts that occurred during the Red Scare of the early Cold War period, when some members turned against (and "turned in") others. What emerges from this sorry episode is an appreciation for the imperative of a viable women's movement to underpin whatever issues women chose to address politically. The ability of female pacifists to endure attacks during the 1920s cohesively and courageously, as well as the broad commitment and sophisticated theoretical exploration of antiwar feminists today, underscores this mutual reinforcement.

One hopes that Alonso will next address the historical dilemma and paradox of broad-based female support for America's wars abroad as well as current feminist efforts to enhance women's roles in the military.

LOIS SCHARF
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SUSAN LYNN. *Progressive Women in Conservative Times: Racial Justice, Peace, and Feminism, 1945 to the 1960s*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. xi, 218. Cloth \$40.00, paper \$15.00.

Susan Lynn's book is a study of two distinctly different organizations, the YWCA and the American Friends Service Committee (AFSC), from 1945 to the 1960s. The subtitle of the work indicates the author's emphasis on three issues: racial justice, peace, and feminism.

Taken as a narrow study of the era, the work has some very insightful and interesting aspects. Of greatest importance is Lynn's analysis of race, both as a public cause and as an issue within each group. The YWCA and the AFSC each sought to racially integrate its leadership and membership while also being involved in the wider civil rights movement. As Lynn illustrates through oral histories and organizational records, the YWCA was much more militant and successful in becoming multicultural than its counterpart, the YMCA. The AFSC also played an important role in civil rights activities while building its own interracial base.

Lynn's comparison of each organization's stance toward feminism is also enlightening. Again through oral histories and organizational records, she shows how the YWCA, as an all-female group, took a more feminist position on several issues than the AFSC, a mixed-gender group. The YWCA, for example, consistently addressed the rights of women workers and steadfastly argued against inequality in politics. In addition, the YWCA also supported married women's right to work during the 1950s, an era marked by society's acceptance of women's return to domesticity after World War II. The AFSC, although welcoming women into its ranks, usually saw them reach only mid-level leadership positions. In this case, traditional gender roles held fast.

As a historian of women's peace activism, I found the section on peace rather weak. Although Lynn mentions such issues as conscientious objection and the nuclear arms race, she does not try to place either organization within the broader movement history. SANE, the Women's International League for Peace and Freedom (WILPF), and the Congress of American Women, for example, suffered severe disruption during the McCarthy era. Both WILPF and Women Strike for Peace were major contributors to the 1960s campaigns to stop the nuclear arms race and the Vietnam War. Yet the reader gets no sense that there was a wider peace movement, especially one embracing women only. This narrowness is also a problem in Lynn's introductory chapter on the interwar era. There is too little historical placement of the YWCA or the AFSC within the wider peace and civil rights movements.

Still, Lynn's volume is an important contribution to the study of women's organizational history, especially as it opens up questions of race relations, a topic

usually downplayed. It also adds to our understanding of women's political activism during the very misunderstood 1950s.

The book has excellent detailed endnotes, but it is unfortunate that Rutgers University Press did not include a bibliography or photos.

HARRIET HYMAN ALONSO
Fitchburg State College

SUZANNA DANUTA WALTERS. *Lives Together/Worlds Apart: Mothers and Daughters in Popular Culture*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xiii, 295. \$25.00.

As the epigraph for her final chapter, Suzanna Danuta Walters quotes Luce Irigaray: "The mother/daughter, daughter/mother relation constitutes an extremely explosive core in our societies. To think it, to change it, leads to shaking up the patriarchal order" (p. 226). Taking up this challenge on behalf of American women, Walters turns to popular cultural images that she claims, "both reflect and construct . . . mainstream ideologies" of mother-daughter relationships (p. 11).

According to these ideologies, the relationship is inevitably fraught with conflict. Intensely bonded with their mothers from early childhood, daughters cannot achieve maturity without separating—often violently—from that primal engagement; many daughters (and some mothers) must rely on male allies to break free. To Walters, this polarization between bonding and autonomy is false, a patriarchal construct perpetuated not only by popular culture but also by psychological theories, even feminist ones. She seeks to "denaturalize" the image of conflict and replace it with a feminist model that depicts continuity, mutuality, and support between generations of women.

Moving chronologically from the 1930s to the present, Walters focuses on the form of popular culture dominant in each era, alternating between movies, television sitcoms, and fiction, with occasional detours into magazine articles and self-help literature. The dynamics of mother-daughter conflict vary over time, ranging from Bette Davis struggling to escape her haughty, repressive mother in *Now, Voyager* (1942) to Winona Ryder seeking a modicum of security from her kookie, self-absorbed single mother (Cher) in *Mermaids* (1990). Mothers such as Margaret Anderson in the 1950s sitcom *Father Knows Best* avoided open warfare with their daughters but served as models of submission to patriarchal norms.

Walters praises exceptional glimpses of mother-daughter mutuality: *A Tree Grows in Brooklyn* (1943), *I Remember Mama* (a 1948 film and then a long-running TV series), and the early 1970s sitcom *Maude*. It is only in African-American women's writings, however, that she finds a distinctly different pattern appearing with any consistency. Writers like Toni

Morrison and Gloria Naylor, by locating women's struggles within specific historical contexts, not only avoid reducing conflict to a psychological inevitability but also succeed in imagining relationships based in respect, understanding, and acknowledgement of the past that more closely approach Walters's own feminist ideal.

At the outset, this study seems to claim that an examination of cultural representations of mother-daughter relationships can offer us a history of actual lived relationships because they are more or less transparent; that is, culture teaches women how to behave. Cultural theorists have long wrestled with the vexing question of reception—the impact of culture on its audience—but for Walters this appears to be unproblematic. She makes no effort to specify particular audiences for particular texts (popularity in itself seems to be sufficient justification for assuming influence), much less to document her assertion about the power of cultural prescriptions. Also left unresolved are such troubling issues as how audience members might deal with competing messages from different texts, and how class, race, ethnicity, and other social cleavages affect reception.

As she moves into the heart of the study, however, Walters abandons her strong claims about the impact of popular culture in favor of simply examining representations in themselves. Rather than analyzing specific texts, she wants to reconstitute "discourses of mothers and daughters" (p. 17). Thus, she strings together cultural examples selected purely on the basis of theme, making little attempt to compare those that deal with mothers and daughters to those that do not. Curiously, she ignores soap operas, both radio and television. Although admittedly daunting in quantity, this is a genre that frequently deals with mother-daughter relationships and surely contributed to discourse formation during the periods under study.

Turning to specific texts, Walters relies chiefly on content analysis, only occasionally referring to deeper textual structures and overlooking entirely the conditions of production resulting from the conscious decisions of producers, directors, editors, and other cultural workers. We are left with the impression of a faceless patriarchal culture that generates discourses about mother-daughter relationships solely to drive women apart. As a historian as well as a feminist (and a mother as well as a daughter), I sense that the cultural process is more complex, and also that the representations of conflict may come closer to lived reality than Walters would care to admit.

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JORDAN A. SCHWARZ. *The New Dealers: Power Politics in the Age of Roosevelt*. New York: Alfred A. Knopf. 1993. Pp. xvii, 411. \$27.50.

Jordan A. Schwarz is certainly well-equipped by background and accomplishment to write a general book about the New Deal. The author of a study of Congress during Herbert Hoover's presidency, as well as the biographer of Bernard Baruch and Adolf A. Berle, Jr., Schwarz sees a need for a synthesis of recent interpretations of New Deal political economy. This book is not that synthesis. Rather it is a "spin-off," and some "spin-off" it is, encompassing well over three hundred pages of text with fifteen announced sketches of "New Dealers" and many others woven in. The result is a scholar's popular history, intended at least in part for the general reader, engagingly written and full of colorful stories.

Schwarz acknowledges that his cast of characters is somewhat arbitrarily chosen. He explicitly excludes members of Franklin D. Roosevelt's first Cabinet as well as New Dealers primarily associated with what he terms "welfare" policies. Rather, his selection is focused on those geared to "financial strategies," especially New Dealers interested in using public credit and investment to promote the economic development of the American South and West. Under the same rubric another writer could have chosen rather different New Dealers and advanced somewhat different themes.

Schwarz's governing concepts involve what he calls "state capitalism," used interchangeably with "public investment," and "state cartelism." The first theme gets considerably more attention than the second. It was the stronger of the two, Schwarz believes, was geared to growth rather than to stability, and won out in the end particularly as public investment was tied to the emergence of massive military spending from 1940 on. I would suggest that stability was not necessarily linked to "cartelism": what, for example, if Schwarz had chosen as his "first New Dealers" publicly oriented planners rather than National Recovery Administration-style "cartelists"? Furthermore, many New Dealers fused rather than dichotomized stability and growth, seeing them as mutually reinforcing rather than contradictory. And leaving out "welfare" appears here to diminish reform, nonregional redistribution, and social justice as major concepts and concerns of New Deal political economy.

Schwarz's biographical sketches illustrate his themes. Each chapter, despite the biographical heading, tends to be far-ranging rather than narrowly focused on the declared subject. The section on David Lilienthal constitutes virtually a history of public power, that on Sam Rayburn tells the story of the Rural Electrification Administration, and Lyndon Johnson is treated as a transitional figure between New Deal and World War II–Cold War versions of public investment. More dubiously, William G. McAdoo, Hoover, and Baruch are styled as "first New Dealers" of a kind, although "McAdoo was never a Roosevelt New Dealer" (p. 30), and Hoover was "not a New Deal President" (p. 47). Similarly, Jesse Jones, a prime focus with his Reconstruction Finance Cor-

poration, was not considered a New Dealer by many contemporaries, Schwarz grants, and the same may be said of Baruch. There are also sketches of Hugh Johnson, Louis D. Brandeis, Felix Frankfurter, Thomas Corcoran, Jerome Frank, William O. Douglas, Wright Patman, and Henry J. Kaiser.

One might quibble about how Mordecai Ezekiel spelled his name, what Al Smith wanted as an issue in 1932, the regional identity of Nevada, and who put Dante into a Roosevelt speech. But all caveats aside, one must give Schwarz his due. He has provided an immensely readable volume, one that should engender renewed historical interest in the New Deal. After all, "public investment" was a major aspect of Bill Clinton's 1992 campaign, one clearly consistent with that strand of the New Deal's tradition and legacy. In this sense, Schwarz concludes, Americans "are still capable of being New Dealers" (p. 350).

THEODORE ROSENOF
Mercy College

RICHARD B. SHERMAN. *The Case of Odell Waller and Virginia Justice, 1940–1942*. Knoxville: University of Tennessee Press. 1992. Pp. xiii, 264. Cloth \$44.95, paper \$19.95.

Richard B. Sherman notes in the preface of this monograph that Odell Waller "is not a familiar name in American history" (p. x). But the events surrounding him have historical significance: the arrest, conviction, and execution of Waller raised questions about justice in the United States, events that "came to symbolize for many the deep-seated racial and economic injustices that . . . deformed much of American society" (p. 1).

The events were largely played out in Pittsylvania County, which is situated in the Piedmont region of southern Virginia. Oscar Davis, the white farmer whom Odell Waller shot, and Waller "lived in a society that condemned many of its members to a life of economic marginality and that maintained racial barriers that for generations had impeded progress in the region" (p. 5).

The lives of the two central figures were drawn together in January 1939 when Odell Waller, his mother Annie Waller, and his new bride Mollie Waller began working as sharecroppers for Davis. From mid-April to July of 1940, Odell Waller worked for Percy Dalton, who built electric lines in Denton, Maryland. Mollie Waller and Annie Waller remained on the Davis farm. Reports reached Odell Waller that Davis had refused to turn over one quarter of the wheat harvest as the sharecropping agreement had stated. Waller returned home on the weekend of July 12 to find out what had happened to his wheat. On Monday morning, Waller visited the Davis farm, accompanied by two friends and several family members. In the presence of Henry Davis, Waller and Oscar Davis spoke briefly before Waller pulled out a

gun and shot Davis, then fled, finally being apprehended in Columbus, Ohio. Efforts to prevent Waller from being extradited back to Virginia failed.

A grand jury indicted Waller. Two organizations came to Waller's defense: the Revolutionary Workers League (RWL), which espoused revolutionary Marxism, and the Workers Defense League (WDL), a militant, nonpartisan body which devoted itself exclusively to the protection of labor's rights. Despite the fact that the RWL withdrew from the judicial aspects of this case, it mounted a campaign to transform the "case from one of merely local interest to one of national significance, in which Odell Waller became the symbol of racial and economic injustice in the United States" (p. 33).

Waller was convicted and sentenced to death in the electric chair. Appeals to state and federal courts did not alter the sentence. One final attempt was made to spare Waller's life when a group of distinguished citizens appealed to Virginia Governor Colgate W. Darden, Jr., to grant Waller executive clemency. Darden consented to hold a hearing, but in the end decided against commutation. Odell Waller was executed on July 2, 1942.

There is one flaw in what is otherwise an important study. Sherman fails to focus on Odell Waller's innocence or guilt. Instead he raises questions about the constitutionality of the trial itself, and on the failure of the judicial system to rule on the question of whether non-poll tax payers had actually been systematically excluded from jury service in Pittsylvania County. Sherman's monograph illuminates deep-seated issues—of poverty and helplessness, race, and power. It is an important chapter in American history and a study that makes one individual a symbol of the broader racial and economic problems that countless numbers of African Americans in Virginia and elsewhere faced on a daily basis.

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CONRAD C. CRANE. *Bombs, Cities, and Civilians: American Airpower Strategy in World War II*. (Modern War Studies.) Lawrence: University Press of Kansas. 1993. Pp. xii, 208. \$29.95.

This is a cogent and superbly researched rebuttal to those who claim American air strategy in World War II was inherently indiscriminate or immoral. Conrad C. Crane, an army officer with a doctorate from Stanford, argues that air leaders were aware of both the limitations of their weapons and the imperatives of their consciences. They followed the public and their political superiors, however, into a type of brutal, total warfare similar to traditional combat on land and at sea.

Crane provides an overview of American air doctrine before the war, as well as the views of political leaders, the public, and aircrew members regarding

strategic bombing. These chapters suffer from oversimplification and generalizations but do present a useful foundation for the description of the bombing campaigns against Germany and Japan. Airmen postulated that strategic bombing could cripple an enemy's industry and thereby shorten the war. American and British air leaders differed over how to achieve this, however, and Crane shows convincingly that U.S. airmen were strongly opposed to the urban area bombing practiced by the Royal Air Force. Although Crane is not certain if this attitude was prompted by humanitarian concerns or a desire to avoid bad press, the result was a determined effort to employ "precision" bombing against military objectives. In truth, airpower was not an overly precise instrument in World War II, although it was far more discriminate than the blunt instruments of blockade, siege, and scorched earth warfare.

The air war against Japan was another story. For reasons not completely clear—although long distances, unusual weather phenomena, and racism were partial explanations—American airmen quickly jettisoned the doctrine of precision bombing they had clung to in Europe and adopted city-busting tactics. Crane notes that the vast majority of the American public, as well as President Franklin Roosevelt and his chief advisers (the seemingly lone exception was Secretary of War Henry Stimson), had few qualms about area bombing of Japanese cities, seeking primarily to end the war quickly. Battlefield casualty rates had quadrupled during the Iwo Jima and Okinawa campaigns, and an invasion of the home islands was dreaded because of the projected cost. America therefore felt more relief than guilt when invasion became unnecessary due to the incendiary and atomic air attacks. Even so, airmen attempted to cushion their blows. Leaflet drops warning the population to evacuate the cities were enormously successful: over six million Japanese civilians fled to the countryside.

Crane's stated intent is to show how and why American airmen had trod the slippery path to total war, and to offer hope of preventing such a journey in the future. He does an admirable job, and, indeed, the prognosis is bright. Airpower in the Gulf War was amazingly accurate and measured, so much so that similarly low levels of collateral damage will probably be expected in future conflict. Overall, this is an excellent book: must reading for anyone interested in the subject of air warfare in general or its moral aspects in particular.

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KIRK FORD, JR. *OSS and the Yugoslav Resistance, 1943–1945*. (Texas A&M University Military History Series, number 28.) College Station: Texas A&M University Press. 1992. Pp. xiii, 249. \$39.50.

In his chronicle of the relations between the Office of Strategic Services (OSS) and the resistance in wartime Yugoslavia, Kirk Ford, Jr., has failed to understand that one branch, at least, of that resistance was headed by a gentlemanly dinosaur. Loyal to the allied cause, Dragoljub Mihajlović nevertheless wanted a Greater Serbia to dominate Yugoslavia; he wanted a return of the monarchy; he abhorred communism even more than he feared Adolf Hitler. Like Mihajlović, Ford seems to believe that communism was a fate worse than death. But to argue, as did Major Scott Dickinson of the OSS in 1945, that Mihajlović was fighting "for a free democratic Yugoslavia" (p. 172) is to ignore both Yugoslavia's interwar history and the popularity of the partisan movement.

Ford has, assuredly, mined the OSS archives and illustrated division within U.S. policy in general and the OSS in particular. He has, however, done so without generating a central thesis on which to hang all of that intriguing information. Unless, of course, a rather uncritical defense of "dinosaurs" qualifies as a central focus.

Both the book and its topic are plagued by subtle distinctions and periodic distortions. Sometimes even identifying the players is a challenge, as it was for OSS observers during the war. We are told that Mihajlović, as a Chetnik, is not to be confused with "the 'Chetnik' umbrella" of variegated Serb war lords who "were, in fact, collaborationists" (p. 129). We also learn that Mihajlović was a Nationalist. What Ford does not make clear is that as a Nationalist Mihajlović was pro-Serbia, while nationalists of the time (the capitalization is crucial) could have been pro-Yugoslavs, pro-Partisans, and even procommunists.

Was Mihajlović a collaborator? Mihajlović and his men "did not cooperate with Axis forces in order to perpetuate Axis domination of Yugoslavia; they did so to prevent its communization by the Partisans" (p. 171). That distinction may help explain Ford's championship of the general's cause, but it also explains that general's later death by a Partisan firing squad.

When Mihajlović stopped fighting the Germans after promised Allied supplies failed to arrive, Ford seems to accept the general's logic—and to blame the British for letting an ally down. He is less forgiving of the Partisans' reluctance to go full out against the retreating German Army late in the war. Nor were the Partisans acceptably grateful for the Allied supplies they received. Ford quotes an OSS official: if "they were able to shift for themselves, we would be out on our ear, without delay" (p. 148). Ford himself seems to find reprehensible the fact that "even under the most adverse circumstances, Tito had been impervious to Allied pressure" (p. 146).

As is true today, wartime Yugoslavia was a very complex and, especially for outsiders, confusing place. Ford is fully aware of that complexity, but clearly he has his favorites. For example, he devotes a fifteen-page chapter to Mihajlović's rescue of some

300 airmen, and he believes, as did part of the OSS at the time, that this should have justified a much greater Allied effort on the general's behalf. The Partisans' rescue of over 700 airmen—who had equally good things to say about their rescuers—gets a belated three-page reference and does not seem to have enhanced Tito's image one iota in Ford's eyes.

LINDA KILLEN
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BETH BAILEY and DAVID FARBER. *The First Strange Place: The Alchemy of Race and Sex in World War II Hawaii*. New York: Free Press. 1992. Pp. ix, 270. \$22.95.

Beth Bailey and David Farber present the territory of Hawaii as a rich field for exploring aspects of the social history of World War II. Hawaii experienced the war more directly than any other American community. It occupied a forward area both in its proximity to the fighting and in its higher threshold for racial and cultural mixing that prefigured the more fluid racial and cultural boundaries of the postwar mainland. Mining censorship reports and other official documents as well as diaries, memoirs, and oral histories, Bailey and Farber have written an absorbing account of how the war influenced self-identities and human contacts across racial, ethnic, and gender categories.

Bailey and Farber have chosen to dig deeply into selected subjects through individuals' stories rather than to provide a comprehensive overview. Each of the book's five chapters treats a particular topic: Hawaii under the influx of more than a million servicemen; the diverse backgrounds, motivations, and experiences of civilian and military newcomers; life in the Hotel Street district where GIs pursued prostitutes and entertainment; the experiences of African Americans; and male-female relationships.

The authors' approach gives readers a profound and immediate sense of how ordinary people confronted the wartime upheaval, but it leaves several topics incompletely examined. We learn, for example, precisely how prostitution operated (three dollars for three minutes, in assembly line fashion) and how the military contravened national policy directing the shutdown of brothels serving GIs. The book takes us into the emotions and experiences of the distinguished 369th Coast Artillery Regiment, the "Harlem Hellfighters," but reveals less about how southern African Americans encountered Hawaii. The chapter on women discusses the extraordinarily lopsided sex ratio and interracial relationships but does not explore women's labor force participation and its potential impact on gender relations. Hawaiians of Japanese ancestry, the largest group at 37 percent of the population, receive less sustained attention, although the authors note their avoidance of mass internment, their vulnerability to violence at the hands of some

GIs returning from the most brutal fighting, and their elimination of traditional elements of Japanese culture.

Martial law ruled Hawaii for most of the war, and the authors demonstrate how federal power sometimes abetted people pushing against the boundaries of difference and prejudice. For example, the military refused to let prostitutes hike prices to five dollars, but it supported them against local efforts to confine their living quarters to the red-light district. Military authorities also backed African-American servicemen who insisted on their right to be saluted by lower-ranking whites, refused to doff hats for southern officers, or relied on self-defense against flagrant racism. The wartime influx of people from the mainland brought more blatant racism to Hawaii, but the war also fostered black resistance: Hawaii's first NAACP branch was formed in 1944, largely by servicemen.

The qualitative nature of their research and the complexity and variety of human relationships across race, ethnic, and gender lines prevent the authors from making broad generalizations. Nor do they go beyond V-J day to draw conclusions about the lasting impact of the war on Hawaii's social fabric or on the people who passed through. Although not a definitive history of wartime Hawaii, this study adds remarkably to our understanding of World War II and of multicultural relations, while its witty and moving prose makes it a delight to read.

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Columbus

ROBERT C. COTTRELL. *Izzy: A Biography of I. F. Stone*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. xii, 388. \$25.95.

Robert C. Cottrell's reworking of his dissertation, "Wielding the Pen as a Sword: The Radical Journalist, I. F. Stone" (1983)—to which the author refers neither in the introduction, bibliography, nor anywhere else—tells the story of a journalist who made his most distinctive mark as the editor and publisher of his own newsletter, the *I. F. Stone Weekly*, from 1953 to 1971. The strengths of the book are the result of extensive interviews that Cottrell conducted with Stone and Stone's friends and family, and the thoroughness of his reading of Stone's reports, essays, and books. Cottrell provides a reliable guide to Stone's long career as a journalist and Washington correspondent for major East Coast newspapers, chiefly in Philadelphia and New York, and his complex relationships with communism (particularly with the American Communist Party), Palestine, Zionism, and Israel. He delineates Stone's unchanging commitment to finding a socialism that could function within a democratic context, and his exasperated but always passionate love of the United States. Cottrell is

at his finest when he handles that period of Stone's life that is best known, that of the *Weekly*, when Stone's voice in the wilderness stood as a beacon for many of those who despaired of the possibility of a progressive politics in Cold War and McCarthyite America, and which provided a crucial link for the members of the Students for a Democratic Society in the 1960s to an older Left politics.

Unfortunately, Cottrell's narrow focus on the activities of his hero severely limit the book's utility as a work of history, for Cottrell fails to make even a perfunctory attempt to place Stone in some larger historical context. One looks in vain for a discussion of Stone's relation to the history of journalism, especially radical journalism, or to the history of American socialism or to American social or intellectual history, or even to Stone's own social background, growing up as a Jew in the Philadelphia area in the first quarter of the twentieth century. When he ventures out of the strictly Stone-based material, Cottrell makes errors of fact and interpretation. Often his attempts at placing Stone in context are limited to lists of Stone's friends and colleagues. One wonders about the presumed audience for the book: confronted with long lists of unidentified persons, is one to assume that all readers will be able to make the connection between them and Stone? Because Cottrell avoids analysis, he fails to ask several obvious if difficult questions: what might it mean that Stone was most effective when progressive politics seemed to be at their weakest? Why did Stone become a "celebrated" American, and what relationship did it have to the decline in the importance of the written word and the rise of television? How can one connect Stone as successful businessman and traditional family man with the radical journalism that he practiced and the calls he made for radical social change? The thoroughness that Cottrell applied to reading the work of Stone is sadly not matched with a scholarly assessment of Stone's place in American history.

ELLIOTT SHORE
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GEORGE F. HOFMANN. *Cold War Casualty: The Court-Martial of Major General Robert W. Grow*. Kent, Ohio: Kent State University Press. 1993. Pp. xiii, 251. \$26.00.

One of the most chilling features of America's Cold War paranoia was the almost random way in which the internal security state selected its victims. They were not all former communists and political dissidents, or even labor leaders and liberal intellectuals. Robert W. Grow may have been the most unlikely security risk of all. During World War II, Grow commanded the Sixth Armored Division in George S. Patton's Third Army. After effective service as the American military attaché in postwar Iran, Grow was assigned to the U.S. embassy in Moscow. As did many

officers, Grow kept a diary. In September 1951, while the general was staying at the Victory Guest House in Frankfurt, West Germany, a Soviet agent on the hotel staff apparently found and copied Grow's journal. Shortly thereafter, East German authorities published extracts from the diary that portrayed Grow as a "warmonger" plotting a preemptive strike against the Soviet Union. Those claims initially attracted little attention on either side of the Iron Curtain, but when the *Washington Post* ran a front-page story on the affair in March 1952, the U.S. Army quickly found itself embroiled in controversy. Within months, Grow was court-martialed for "failing to properly safeguard classified information."

In the first full-length treatment of Grow's case, George F. Hofmann rescues a disturbing slice of Cold War history from an undeserved obscurity. Grow fell victim, Hofmann argues, to a variety of forces: improper meddling in his case by senior officers, political pressures stirred up by careless journalists, and, "most important, the inherent tensions caused by the temper of the times" (p. 6). According to Hofmann, intelligence experts disagreed as to whether Grow's diary actually contained any sensitive information, and the published quotes accusing him of advocating war were either forged or taken out of context. Grow's superiors, including Army Chief of Staff J. Lawton Collins and his deputy, Maxwell D. Taylor, decided that punishing Grow would be more politically expedient than trying to refute the communist propaganda. Fearing exposure of lax security at the Victory Guest House, a private hotel commandeered by U.S. occupation forces, the army also prevented a thorough investigation by Grow's lawyers of the circumstances surrounding the copying of his diary. The result was a proceeding with all the shortcomings typical of security investigations of the period: lack of adequate pretrial discovery, denial of the right to face one's accusers, absence of independent appellate review, and more.

This study, however, deals less with McCarthyism than with the problem of "command influence," the army's euphemism for the tendency of the generals to run military courts as their personal fiefdoms. Hofmann follows a number of themes, but none is more intriguing than his suggestion that the expansion of the armed services during and after World War II put a premium on bureaucratic and managerial skills at the expense of honor, bravery, and traditional military virtues. Grow, Hofmann implies, was sacrificed by a careerist high command more interested in personal promotions than battlefield victories. Despite a few flaws—Grow's character, for example, is never fully developed—Hofmann's study, prodigiously researched and richly detailed, is quite impressive.

JEFF BROADWATER
Barton College

LUDWELL LEE MONTAGUE. *General Walter Bedell Smith as Director of Central Intelligence, October 1950–February 1953*. Foreword by BRUCE D. BERKOWITZ and ALLAN E. GOODMAN. University Park: Pennsylvania State University Press. 1992. Pp. xxviii, 308. Cloth \$45.00, paper \$14.95.

Ludwell Lee Montague's in-house history of the CIA under Walter Bedell Smith was written after Montague's retirement from the CIA in 1970 and completed in December 1971. It reflects orthodox CIA doctrine on Smith's transformation of the weak and doomed agency under the first three directors into the powerful intelligence gathering and covert activities force that Smith handed over to his successor, Allen W. Dulles, in 1953. Before accompanying Smith to the CIA when General Smith became the agency's fourth director in 1950, Montague had been a staff officer for the Joint Chiefs of Staff. There Montague drafted the proposal—JIC 239/5, (January 1, 1945)—that eventually led to the CIA's establishment in 1947. Until his retirement in 1970, Montague served on the CIA's Board of National Estimates, responsible for developing the agency's official evaluation of international situations.

Montague wrote this volume—exclusively an administrative study of the Smith years—to refute an earlier (1953) in-house history of the pre-Smith CIA by Arthur B. Darling. Darling's history was suppressed by then-Deputy Director Dulles for defending the record of the pre-Smith directors and not giving sufficient credit to Smith and the Dulles-Jackson-Corea report of 1949 that led to the Smith reforms.

It is small exaggeration to say that the point of Montague's book is the contrast between two organizational charts he reprints, one representing the agency that Smith found, and the other the one that he left. Montague's hermetically sealed account of administrative changes within the agency and bureaucratic turf battles with national security rivals will induce claustrophobia in anyone interested in what the agency was doing outside Washington. It does underscore a reality too seldom appreciated by historians without experience in government service: it is perennially tempting to inexperienced or public relations-oriented politicians to deal with problems on the cheap by setting up task forces of existing agencies, but such cooperative ventures so violate the sound management principle of an unambiguous chain of command—particularly in military or quasi-military organizations—that the existing agencies protect their autonomy by doing nothing. Thus, either nothing is accomplished, or else the task force has to develop its own facilities for accomplishing its assignment on its own. Smith did the latter, making the director of Central Intelligence the master of his own house with his own intelligence gathering and covert activity capacities, so that he was no longer simply a coordinator of information furnished by

sources outside his control, a situation that had defeated his three predecessors.

After the CIA's release of Montague's text to the National Archives in 1990, Bruce D. Berkowitz and Allan E. Goodman, both formerly with the CIA, supplied a well-informed introduction and had it published by Pennsylvania State University Press, which had also published Darling's book (*The Central Intelligence Agency: An Instrument of Government, to 1950* [1990]) under Berkowitz's and Goodman's supervision. All involved have performed a valuable public service.

RICHARD GID POWERS
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City University of New York

CRAIG ALLEN. *Eisenhower and the Mass Media: Peace, Prosperity, and Prime-Time TV*. Chapel Hill: University of North Carolina Press. 1993. Pp. 259. Cloth \$39.95, paper \$14.95.

Craig Allen demonstrates that Dwight Eisenhower—and not his far more telegenic successor—was the first chief executive to attempt to use television to maintain his control over public opinion. Despite some weaknesses, his study should be read by all who study the history of the presidency and television.

Allen sees several explanations for the first "television presidency." As a military leader in the 1940s, Eisenhower had taken public relations seriously; he worked hard and effectively to establish cordial relations with journalists. Yet he enjoyed much less success wooing the White House press corps. Occasionally critical coverage caused the new president to regard correspondents as obstacles to his leadership of public opinion. So were many in the Republican Old Guard who challenged Eisenhower's handling of domestic subversion and foreign policy. Television, Eisenhower and his aides determined near the end of 1953, would allow the president to overcome both the Old Guard and the fourth estate.

The president and his advisers began utilizing television to advance the administration's cause. These included televised messages and "photo opportunities" for the new TV news programs. In 1955, Eisenhower permitted his press conferences to be telecast, although not live; press secretary James Hagerty edited the films prior to their release. The overseer of these activities was actor and producer Robert Montgomery, whose significance was denoted by his coveted White House office assignment.

Allen is generally cautious in assessing the effects of Eisenhower's TV strategy. The audiences for television news and especially the broadcasts of his press conferences were modest. Yet, to Allen, Eisenhower's fairly consistently high public approval ratings were no accident, but part of a near constant public relations effort.

Still, this study is best used as a guide to what

Eisenhower and his circle, as well as Democratic leaders during the 1956 campaign, thought about TV's political potential. And that strength is also a weakness. Relying heavily on White House sources, Allen frequently takes up their point of view. A Christmas Eve 1953 broadcast, he writes, "had been a big success" (p. 28). For whom? Montgomery liked it, but no other measure of "success" is presented. Allen should have leaned less on some individual memories and reviewed more of the secondary literature. An interview is cited for a statistic (incorrect) on the percentage of newspapers endorsing Eisenhower in 1952; *Editor & Publisher* carried the exact figure near the end of every campaign. A long analysis of the *Sputnik* launch is not informed by Walter McDougall's insightful study, which included a chapter on the mass media.

Allen's failure to discuss certain TV appearances by Eisenhower is baffling. Absent is any mention of the president's 1953 speech to the Anti-Defamation League, aired by three networks, and which challenged the worst excesses of the anticommunist Old Guard. A similar omission spoils an interesting chapter on Eisenhower in retirement. Allen describes the former president's work as a commentator for ABC News at the Republican convention of 1964, but not his speech to the delegates, which included a well-received denunciation of the press. In other instances, Allen simply falls into the habit of emphasizing the production of a telecast and not its content. What did the president say in his address during the desegregation crisis of 1957 at Little Rock? Even in the age of television, words could matter.

JAMES L. BAUGHMAN
University of Wisconsin,
Madison

ROBERT A. DIVINE. *The Sputnik Challenge*. New York: Oxford University Press. 1993. Pp. xviii, 245. \$25.00.

This study by Robert A. Divine is a useful illumination of the American reaction to the sudden, unexpected Russian achievement in launching the world's first artificial earth satellite, or "*Sputnik*." Dispassionate, balanced, and well researched, Divine's book also makes a contribution to the understanding of internal American political decision making of the Eisenhower administration. In a broader sense, the book helps explain how Americans understood themselves midway into the Cold War.

Some readers will be surprised to learn that officials in the Eisenhower administration were aware in advance of the Soviet program to launch a satellite, and that the president deliberately decided not to compete by rushing an earlier American launching. An American satellite could have been launched first by using a military launcher (the Jupiter missile), rather than waiting for the civilian Vanguard space system to be ready. In retrospect, in light of the political

impact of the Soviets' launching of *Sputnik*, President Eisenhower's decision not to launch the first satellite was clearly a mistake.

The *Sputnik* challenge had several major aspects. In particular, it contributed to what became a major issue of American security policy, the "missile gap," a fear from 1958 to 1961 that within a few years the Soviet Union would have hundreds, then thousands of intercontinental ballistic missiles (ICBMs), while the United States was expected to lag badly with only dozens to a few hundreds. This became a major political issue exploited by the Democrats in the 1958 congressional elections and especially the 1960 presidential campaigns. Eisenhower was right in rejecting the idea of a burgeoning Soviet threat to American security. The missile gap never materialized. Yet he was unable to reassure "a badly shaken nation" or dispel the image of the gap. As Divine notes, Eisenhower decided against disclosure of the top secret U-2 reconnaissance overflights of the Soviet Union that would have helped to counter the notion of a missile gap. Yet, as he also correctly notes, such disclosure and even revelation of the information obtained would not have been conclusive in the public debate of 1958-60, just as it was not within the intelligence community. Eisenhower depended more on his own experience and intuition, ultimately more on his evaluation of Soviet intentions, than on Soviet capabilities to build missiles.

Apart from the "missile gap," the *Sputnik* shock stirred Americans into reevaluating its educational system and helping its own space program. Eisenhower was skeptical of the need for any basic change in education. There were steps to stimulate education in the sciences, but this did not become a divisive political issue. More broadly, the *Sputnik* challenge jarred American complacency over assumed superiority of our society in general; not enough to cause basic doubts or radical change, but enough to serve as a beneficial reminder that America was not inherently or always "first."

There are a few errors in this book. There was no deployment of Soviet ICBMs detected "in an arc along the trans-Siberian railroad" in 1960 (p. 183), and the "fewer than a dozen" ICBM launchers in 1960 (p. 195) were in fact only four in 1960-61, at Plesetsk in the far north. The national intelligence estimates of 1958-60 overstating expected Soviet missile deployment should not be attributed only to CIA, but to the intelligence community as well. CIA (internally divided) took a position between higher air force and (sometimes) State Department estimates and lower army and navy ones. But these are minor blemishes on a solid, worthwhile, and interesting study.

RAYMOND L. GARTHOFF
Brookings Institution

STUART W. LESLIE. *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and*

Stanford. New York: Columbia University Press. 1993. Pp. xiii, 332. \$42.00.

This is a book whose main title unfortunately promises more than it delivers. Readers expecting a broad study of the impact of the Cold War on American science will be disappointed. One wishes that Stuart W. Leslie had been more ambitious, for this well-written, detailed study of the development of the military-industrial-academic complex at MIT and Stanford does provide important insights into the fate of American science during the years of the Cold War. These insights are, however, filtered through the very particular lenses provided by the history of institution-building and discipline-formation at two leading universities. Furthermore, the larger subject of the Cold War culture that so altered the scientific enterprise never emerges in its full complexity.

Leslie's primary concern is how the Cold War "redefined American science" (p. 1) by skewing it toward military interests. Because Stanford and MIT were the most successful at forging an alliance with the growing military-industrial complex and became the models for others to follow, they do provide important case studies. Leslie, however, remains so focused on their individual experiences that he is only partially successful in explaining the broader shift of which they were a part. To some extent this is the result of a perspective that focuses on academic empire-builders and gives military and government policy makers only token roles. Additionally, Leslie only examines certain physical science and engineering disciplines—electrical and electronic engineering, aeronautics, nuclear physics, and materials science—each of which is the subject of paired chapters, one detailing MIT's experience followed by another examining Stanford's. Nonetheless, within the course of these detailed disciplinary narratives, Leslie does make several important points about the changing character of an American science and technology increasingly dominated by Cold War concerns focused on military and space technology.

Leslie argues persuasively that, at least within these disciplines, the Cold War left the scientific community with a "diminished capacity to comprehend and manipulate the world for other than military ends" (p. 9). This occurred because they defined themselves by the needs of their military (and space) patrons who influenced not just how those working in these disciplines applied knowledge but also what knowledge they sought through basic research and what they taught their students. Surprisingly, the teaching function turns out to be an important one for military strategists. Recognizing that science and engineering had altered the nature of warfare, the armed services wanted to insure a continuing pool of researchers who could keep American military technology at the forefront. Graduate teaching as well as research took place in laboratories and research centers established with military support. Within these new academic

institutions researchers from various fields in basic and applied science and engineering came together to solve specific problems. The already close ties between science and engineering were further deepened in this new interdisciplinary context.

Although its institutional and disciplinary focus will make this book of greatest interest to historians of science and technology, those more generally interested in Cold War America may also find it useful. It could also prove significant for those interested in the current debate about the government's role in industrial policy, which, as Leslie rightly notes, has for too long been set by the military.

PAUL ISRAEL
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New Brunswick

PETER DOUGLAS FEAYER. *Guarding the Guardians: Civilian Control of Nuclear Weapons in the United States*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1992. Pp. xviii, 261. \$34.50.

Peter Douglas Feaver has written an analysis of U.S. civilian control of nuclear weapons that will exasperate many traditional narrative historians. Feaver has divided his excellent study into two parts. First, he describes a model for the control of nuclear weapons based on the American presumption of civilian dominance of the military. Second, he uses the model to analyze how and why civilian control of nuclear weapons changed during the Cold War from 1946 to the present.

Feaver's model embraces both custody and use of nuclear weapons. He identifies two patterns of American civilian control: assertive and delegative. Under the assertive pattern, civilians become intimately involved in managing the nuclear stockpile and in planning and authorizing nuclear weapon operations. Not surprisingly, assertive control can lead to intense conflict between civilian and military leaders. Under the delegative pattern, in contrast, conflict lessens because civilians observe a division of labor that relies on professional military responsibility for custody and use of nuclear weapons.

Feaver's narrative chapters explore the history of civilian control of nuclear weapons. Following passage of the Atomic Energy Act in 1946, strict assertive civilian control characterized the Truman administration when the nuclear stockpile was small, international threat was low, and nuclear doctrine was in its infancy. During the Eisenhower administration, however, assertive control gradually declined as the stockpile grew, the Cold War intensified, and nuclear doctrine matured. According to Feaver, since 1960 assertive and delegative control have fluctuated, mostly driven by shifts in nuclear doctrine. Other factors, such as presidential leadership, international crises, and stockpile configuration, have also influenced control patterns. Although Feaver is generally pleased with the

"considerable explanatory power" of his model, he concedes that "complex and intricately shifting patterns of civilian control" need further study as Cold War archives are opened for research (p. 219).

Despite the difficulty of researching this highly classified topic, Feaver's book makes an important contribution to our understanding of the nuclear revolution, both theoretically and historically. Although he confines his analysis exclusively to American history, Feaver believes that his model is generally applicable to all nuclear weapon states. When America's nuclear weapon stockpile was small and the B-29 was the only delivery vehicle, assertive control by the president was possible. With expansion of the stockpile and multiplication of strategic and tactical delivery systems, civilian custody of nuclear weapons became impractical. Ultimately, civilians exchanged custody of nuclear weapons for assertive control of their operational use. Presently, permissive actions links (PALs), authorizing codes which enable the military to arm and fire their nuclear weapons, maintain civilian command and control over the nuclear arsenal.

In charting the history of civilian control of nuclear weapons, Feaver does not confuse custody and use of weapons with the management and control of nuclear science and technology. Ignoring development of nuclear policy by the president and congress, Feaver focuses almost exclusively on control of nuclear bombs and nuclear operations, including command of the nuclear navy. Research, development, testing, and production of nuclear weapons are beyond his consideration, as are problems associated with peaceful technology and nuclear weapon proliferation. Within these limitations, however, Feaver's book will become an important addition to nuclear history's bibliography.

JACK M. HOLL
Kansas State University

ALLAN M. WINKLER. *Life under a Cloud: American Anxiety about the Atom*. New York: Oxford University Press. 1993. Pp. 282. \$27.50.

Allan M. Winkler has written a lively and informative book on an important topic. Although historians of the Cold War have dealt extensively with nuclear crises and confrontations and with the halting efforts toward arms control, they have tended to neglect the fascinating issue of how the American people adjusted to living decade after decade with the knowledge that at any moment life as they had always known it could be obliterated. Winkler helps fill this gap by concentrating on the sporadic movement of concerned scientists, intellectuals, and artists to stimulate public concern and regain control over the atom.

There are many strengths in Winkler's book. By keeping a broad focus and organizing his presentation topically, he offers a sweeping yet coherent

survey in just over 200 pages. He is most effective when he explores the cultural manifestations of public concern over the nuclear dilemma. Winkler uses popular songs, movies, novels, poems, paintings, and television programs to reveal the curious mixture of humor and dread that characterized the way most Americans tried to relate to a world filled with atomic bombs and hydrogen warheads. He explains how schools used the image of a turtle withdrawing into its shell to explain to children the technique for "Duck and Cover" drills and showed Walt Disney's film, *Our Friend the Atom*, to portray the blessings of nuclear power in the days before Three Mile Island and Chernobyl. Tom Lehrer's verses ("So long, Mom, I'm off to drop the bomb, So don't wait up for me") and a brief summary of Stanley Kubrick's *Dr. Strangelove*, ending with the camera panning over a series of mushroom clouds to the strains of "Someday We'll Meet Again," provide a telling commentary on American adjustment to the daily possibility of a nuclear holocaust.

There is just one unfortunate weakness: Winkler's obvious bias. He makes no effort to disguise his admiration for concerned scientists like Linus Pauling and eloquent writers such as Jonathan Schell who tried to arouse the people to their peril; at the same time, he clearly has only disdain for politicians such as Ronald Reagan, who seek to deflect antinuclear sentiment, and for misguided scientists such as Edward Teller, who view programs like Star Wars as gigantic pork barrels to keep their laboratories humming throughout the Cold War. Winkler never balances his view of impending nuclear disaster with a consideration of how these dreaded weapons might have helped the human race. In trying to answer the question of why the people failed to respond to the concerns of those he portrays so nobly, Winkler could have asked if Americans did not instinctively realize that the bombs and missiles had one profound benefit: because they were too horrible to use, nuclear weapons helped spare the world a third great war in the twentieth century.

ROBERT A. DIVINE
University of Texas,
Austin

KENNETH H. HEINEMAN. *Campus Wars: The Peace Movement at American State Universities in the Vietnam Era*. New York: New York University Press. 1993. Pp. xvi, 348. \$40.00.

Kenneth H. Heineman traces the rise of left-wing and peace activism from the beginning of the Vietnam War to about 1972 at four state universities: Michigan State, Pennsylvania State, Kent State, and the State University of New York at Buffalo. Radical dissent, administrative intransigence, and violence was not limited to elite schools like Berkeley and Columbia. State university administrations cooperated with fed-

eral and local authorities, first in military-related contracts and a Cold War version of institutional mission, and then in the surveillance, repression, and harassment of dissenters. Each campus was distinct from the others, although all had some things in common.

The book is divided into analytical and narrative sections. The first part offers an analysis of each university, faculty, and student body, with respect to their institutional and social characteristics. The larger part of the book is a narrative of dissent and administrative responses to it from 1965 to 1970. An epilogue recounts the denouement of student activism and relates narrative to analysis.

Heineman tells his stories well, but the reader has to follow four separate plot lines because all four institutions are treated separately in each of three chronological chapters. There is cohesion in common elements: the drift toward extremism of the radical Left on each campus; the obdurate if not repressive responses of administrations; the intrusion of surrounding communities; and the pattern of spiraling confrontation and violence until spent energy at last revealed the emptiness of both radical antiwar and prowar ideologies.

The narrative traces mounting confrontations that led to brief incidents of uncontrolled violence, rather than systematic "campus wars." Moreover, it focuses on organized campus radicalism, rather than on antiwar protest as a whole. Local radicals, especially in SDS chapters, occupy the foreground (national developments in SDS are not explained). Mainstream, liberal campus dissent recedes into the background. The war itself often pales beside issues such as racism, *in loco parentis*, arbitrary authority, and the threat of the draft. Indeed, Heineman acknowledges that the overwhelming majority of antiwar students were involved only incidentally in local political campaigns and demonstrations, and that liberal groups had far more appeal than radical ones (p. 270 and *passim*).

To some extent the focus on radicals meets the requirements of a cohesive story line, and in some measure it reflects the author's sources. The book is well researched in the relevant secondary literature and in university archives. Student publications, the papers of former activists, and interviews with them add rich detail to the narration. As the author observes, however, "activists from the 1960s have exaggerated their own historical importance" (p. 2). What is at issue is not the veracity of the sources, but rather the weight they give to the most radical and dramatic side of dissent.

The analytical portion of the book describes the vast expansion of state university systems prior to the Vietnam War, often under the aegis of a Cold War mission and in tandem with military research. No doubt that contributed to the character of discontent and institutional responses to it. But a causal relationship is not established on the basis of four case studies, particularly where identifiable activists con-

stituted 1 percent or less of student bodies and 2 to 4 percent of faculties. Indeed, Heineman eschews "sweeping generalizations" on the grounds that (with respect to campus life, at least) "in America all politics really are local" (pp. 265–66).

His most important contribution, then, is to document four dramatic cases in which "the relative success or failure of an antiwar organization often depended upon the cultural ambiance of the campus" (p. 270): the social background of students and faculty; the relative strength of the liberal arts, from which overwhelmingly dissent was drawn; the institutional orientation to change and diversity; campus politics; and the influence of the community environment. The historiographical challenge is to connect such local realities with national movements.

CHARLES CHATFIELD
Wittenberg University

EARL H. TILFORD, JR., *Crosswinds: The Air Force's Setup in Vietnam*. Foreword by CAROLINE F. ZIEMKE. (Texas A&M Military History Series, number 30.) College Station: Texas A&M University Press. 1993. Pp. xvii, 252. \$30.00.

In this volume, Earl H. Tilford, Jr., describes the failure of American airpower from the viewpoint of a historian and former Air Force officer who served during the war analyzing intelligence in Thailand. His book meshes well with Mark Clodfelter's *The Limits of Airpower* (1989), a Clausewitzian critique of the incongruity between American political objectives and air operations. Whereas Clodfelter concentrated on the actions of presidents Lyndon Johnson and Richard Nixon and their senior military and civilian advisers, Tilford focuses on the Air Force. He concludes that if the Vietnam War was primarily an air war, then America's failure to achieve its goals was also a failure of air power: "In other words—words that are anathema to many in the Air Force—we lost" (p. 183).

Tilford argues that the Air Force set itself up for failure in unconventional warfare in Southeast Asia. The service had gained its independence on the promise of strategic bombing, and, because of its interpretation of World War II, national security strategies of the 1950s, and the overriding influence of General Curtis LeMay and his Strategic Air Command, Air Force doctrine stressed the attainment of victory by destroying enemy industrial centers. Strategic bombing doctrine fit well with defense policies such as massive retaliation, enabling the Air Force to gain a generous share of budgets and pursue new technologies that seemed to substitute for strategy and were inappropriate for tactical missions. The technology fostered a managerial ethos that produced a rigid approach to war that confused efficiency with effectiveness. Because aerial warfare was so technical, most civilians, including key advisers in

Washington, did not understand what it could really accomplish, but Air Force generals were not politically astute enough to give presidents useful supplementary advice.

Tilford's book is especially valuable on describing air operations in Laos and Cambodia, including Operation Commando Hunt, the primary air campaign between the gradual escalation of Rolling Thunder and the more successful Linebacker I, which, by aiming to maximize trucks destroyed along the Ho Chi Minh Trail, became another futile exercise in statistics. He covers all major air operations in detail and effectively dispels the "myths" surrounding the "Christmas Bombing," or Linebacker II, which was neither as heinous or decisive as critics or supporters claim. He also argues that personnel policies in the early 1970s led to a decline in U.S. Air Force performance and turned the air war into "a contest between a peacetime air force (American) and a highly motivated wartime air force (North Vietnamese)" (p. 156). At times the North Vietnamese Air Force even achieved superiority in air-to-air combat, and its sophisticated air defenses contributed significantly to U.S. Air Force failure.

Despite his critique of Air Force shortcomings, Tilford also has no real solution to the problem of how airpower could have won in Vietnam. Victory could not have been gained through firepower alone, he concedes, "short of the complete obliteration of North Vietnam," an option never considered by American leaders (p. 192). And although he argues that the Air Force's reluctance to admit defeat has hindered self-examination, he admits that some lessons learned from Vietnam influenced the successful air campaign in the Gulf War. These small flaws notwithstanding, Tilford has provided a fresh viewpoint of how the traditional Air Force—and American—emphasis on technology and strategies of annihilation can sometimes be inadequate for limited unconventional wars, a type of conflict we will likely see more of in the post-Cold War world.

CONRAD C. CRANE
United States Military Academy

WILLIAM L. VAN DEBURG. *New Day in Babylon: The Black Power Movement and American Culture, 1965–1975*. Chicago: University of Chicago Press. 1992. Pp. x, 377. \$29.95.

Dealing in a scholarly manner with recent historical events is seldom easy. Impressions, recollections, and priorities intrude on a narrator's task, readers' responses, and the objectivity and detachment so important to valid analysis. These problems are compounded when the subject matter cuts across sensitive racial, ethnic, or national boundaries where "they" and "we" have been known to govern the dialogue.

To his credit, William L. Van Deburg has surmounted these difficulties to produce an excellent

study of militant African-American culture in those transitional years, 1965 to 1975, that witnessed such staggering changes in modern America. He succeeds because he works from the black community's perspective. While in no way an uncritical glorification of the movement, his discussion of black power has the voice of its advocates and practitioners rather than its detractors.

Van Deburg's credibility derives from a demonstrable familiarity with and sensitivity toward the primary documents generated by black power figures—their speeches, poems, essays, position papers, organizational materials—and from his stunning command of the pertinent secondary literature by journalists, government officials, and social scientists who then and later commented on the black power years. This clearly was not a book done in haste, whether in its conceptual, research, or writing stages.

The author devotes separate chapters to the background and formulation of a black power ethos, the identities and objectives of key personalities and groups within the movement, the diverse ideologies that defined black power, its articulation and functions among African Americans, and the movement's intersections with mainstream America. Van Deburg consistently avoids the melodramatic postures of mass-media critics and of participant-apologists. He keeps his scholarly balance in three ways: by setting black power developments within their historical context, by handling the movement with a straightforward respect that never stoops to condescension, and by orienting the presentation, chapter after chapter, to his central analytical theme of culture as a way of life and self-definition. That theme permits him to discuss and explain an impressive range of topics and events, such as the June 1966 "March Against Fear"; the diverse but not wholly discrete approaches of territorial, revolutionary, and cultural nationalists; the role of violence as self-defense; African-American militancy in the paid work force, on college campuses, and in military and penal systems; artistic and religious codifications of black power concepts; and white America's uncertainties about and manipulations of black power components, to name but a few.

Two shortcomings emerge from the book's strengths. First, Van Deburg's reliance on sources, to the point of almost complete detachment, leaves the reader anxious to learn more directly what the author thinks about events in question. Second, his application of the term "Black Power" occasionally seems too encompassing and thus too imprecise for the complexities being surveyed. Nonetheless, this is a splendid volume whose attention to detail, sound organization, and scope sustain the conclusion that "the central focus of the Negro-to-Black conversion process was not on evaluating white people, but on encouraging black people to reevaluate themselves" (p. 290). As Van Deburg demonstrates through-

out, this was culture in the service of pragmatic necessities.

ROBERT L. ZANGRANDO
University of Akron

FREDRICK B. PIKE. *The United States and Latin America: Myths and Stereotypes of Civilization and Nature*. Austin: University of Texas Press. 1992. Pp. xix, 442. Cloth \$40.00, paper \$19.95.

Fredrick B. Pike has produced an intriguing account of the history of U.S. relations with Latin America. Using North American concepts of nature and civilization as his framework, he presents a unique perspective on those relations, one which cannot be ignored by scholars of this subject.

According to Pike, U.S.-Latin American relations can be understood only by grasping the significance of U.S. conceptions of nature and civilization. For Americans, the march of civilization is predicated on the constant penetration and conquest of nature. Externally, this has meant the subjugation of the frontier, whether in the Far West, the Pacific, or Latin America. In a process that combines elements of G. W. F. Hegel, Charles Darwin, and Frederick Jackson Turner, the civilized American (the "thesis") "plunges into nature, embodying the antithesis." From this "emerges the remade human, the synthesis in which opposites fuse" (pp. 126-27). Internally, Americans have fought a constant battle against "natural" urges such as anger, sloth, and licentiousness. On both the physical and moral levels, American history has been dominated by the will to conquer nature, turning it to more civilized ends, while profiting both materially and spiritually.

This is where Latinos fell short. They, too, had "plunged into nature," but instead of conquering it, had chosen to become part of it, with all the attendant evils. Whether due to religious, cultural, or racial weaknesses, Latinos did not have the requisite fortitude to dominate nature. Whereas energetic Americans plowed their way (literally and figuratively) into the Far West and overseas, accumulating wealth and spiritual renewal, Latin Americans eked out survival and eyed the jungles with awe, superstition, and resignation. Americans, conquering the "internal" frontier, were level-headed, sober, thrifty, and able to adapt. Latin Americans, having surrendered to the external frontier, had even less resolve in combatting their natural instincts. Thus, Pike argues, the stereotype of the temperamental, drunken, tradition-bound Latino easily developed.

With such beliefs ingrained in the American psyche, relations with Latin America were destined to be filled with rancor. The constant search for new frontiers (territorial and economic) brought the United States into continual conflict with Latin America. Believing that Latinos were "wasting" their wealth by not fully subjugating nature, Americans took

stances ranging from arrogant paternalism to belligerent domination. Because the Latinos were seen as being in a state of nature rather than civilization, it was easy enough to view Latin America as a new frontier to be conquered. That region's inhabitants would be uplifted, the conquering North Americans revitalized.

Pike relies on studies from sociology, anthropology, economics, philosophy, religion, political science, and history to make his case for the importance of the nature/civilization dichotomy in American history. In addition, he makes broad but exciting use of literature, art, and music. There are few history books in which you will find William Cullen Bryant, Leonard Bernstein, Bruce Springsteen, Edgar Allen Poe, Herman Melville, and Chuck Berry all used as sources.

Such wide scope and fascination with American culture makes for certain problems. Pike apologizes for his rather scattershot approach, and the reader must be alert to the author's not infrequent digressions. Furthermore, while claiming to be a study of the United States and Latin America, it focuses almost entirely on "myths and stereotypes" formulated by the former.

These are not overwhelming problems, however. Pike's book is not to be quickly digested, cannibalized, and then exiled to the bookshelf. This is a work that deserves to be considered at length and as a whole. If it rambles at times, it remains throughout an exhilarating and thought-provoking journey through U.S. culture, politics, and diplomacy. It provides telling lessons in how the United States sees itself, foreign "others," and the relationship between the two.

MICHAEL L. KRENN
University of Miami
Coral Gables, Florida

RON ROBIN. *Enclaves of America: The Rhetoric of American Political Architecture Abroad, 1900–1965*. Princeton: Princeton University Press. 1992. Pp. xiii, 208. \$24.95.

American architecture is one of the most impoverished fields of scholarship in architectural history, particularly with respect to political issues. Beset by problems ranging from excessive provincialism to excessive formalism to indifference to most of the built environment, the study of American architecture has been remarkably unimaginative. Ron Robin's study of American building overseas not only fills a yawning gap but also offers a model for assessing the political dimensions of government building enterprises.

Unlike other imperial powers in the nineteenth century, Robin argues, the United States lacked a sense of participating in a world community. Reluctantly, intermittently, and under intense pressure, the fledgling nation committed funds to enterprises over-

seas. But the twentieth century was an entirely different matter. Robin's book traces the record of the American presence overseas in cemeteries, war memorials, and embassy buildings. Although the United States intervened in World War I, strong isolationist sentiments persisted throughout most of the interwar period. War memorials to victims of the war serve as telling examples of the conflicting ideas that underlay American attitudes toward entanglements abroad. These monuments at once affirmed a new role in international affairs and implicitly called for special recognition from the host countries for the role of American soldiers in "saving" them. Paul Cret, supervising architect of the American Battle Monuments Commission, brought together time-honored themes—individualism, voluntarism, laissez-faire government—in a series of eight cemeteries throughout Western Europe, with architectural tools employed to downplay individual sacrifice and loss in favor of an all-encompassing common cause.

During the same period, the American government began to construct monumental diplomatic headquarters, functional buildings that were also designed to represent America's international prominence. Economic hegemony rather than warfare and colonialists underlay a brand of imperialism geared toward colonial exploitation and shifting emphases on protectionism and free trade. The embassy buildings that were to express often inconsistent political and economic aspirations themselves were equally inconsistent. From the beginning of the century, embassy and legation designs ranged from a regionally based, classical, and palatial model, to the southern plantation house, to variations on the White House. Robin deftly leads the reader through the ideological debates surrounding each architectural style and weaves the story into a larger narrative concerning America's self-image and developing international presence.

A good example is the shift from the palatial, imperial style. Derided as ostentatious and evincing aristocratic airs, these early embassies gave rise in 1926 to a decision to rationalize architectural styles and produce a visibly American architecture. It should come as no surprise that the style selected had colonial American roots, the southern plantation house. As Robin wryly notes, this image of a society of landed gentry conveniently ignored the basis of that society in slavery.

Robin skillfully mined archives and other public records to demonstrate how the architectural symbolism of buildings and cemeteries reflected uncertain and often incoherent political and economic aims, and how a society that believed itself to be democratic was unable to define a political architecture that reflected diversity rather than a single-minded mission.

Indeed, the very fact that so many of the works examined here reflected precisely a single-minded mission, even if it differed from place to place, eloquently testifies to the often unacknowledged contradictions of that democratic image. This elegantly

argued book challenges historians and architectural historians to address these matters with respect to other government building enterprises.

DIANE GHIRARDO
University of Southern California

ROGER BUCKLEY. *US-Japan Alliance Diplomacy, 1945-1990*. (Cambridge Studies in International Relations, number 21.) New York: Cambridge University Press. 1992. Pp. xiv, 225. \$49.95.

The United States and Japan are the world's technological and industrial superpowers. They have also maintained for more than forty-five years a close and mutually beneficial political and military alliance. Despite differences in industrial organization and disputes over trade, it is generally acknowledged by senior government and business leaders in both countries that the long-term benefits of this bilateral relationship far exceed its costs. The sheer complexity of this vast, multilayered relationship also deserves better than the broad-brush analysis contained in this study; indeed, Roger Buckley describes his work as "no more than an introductory text that sketches how the United States has handled its relations with contemporary Japan" (p. xiii). Unfortunately, the reader will find that even this modest agenda is not adequately addressed.

This is a curious book that purports to examine U.S. policy toward Japan between 1945 and 1990, but which is based largely on U.S. policy papers that cover the period 1945-69. Thereafter, the author relies almost exclusively on secondary works in English. This, Buckley freely admits, renders his "remarks on the 1970s and 1980s" speculative. This imbalance is also reflected in the fact that only sixty pages of text are devoted to the period after 1970. Even more bewildering is the almost complete absence of either a consistent Japanese perspective or Japanese-language source materials.

If the reader is prepared to accept a rather limited analysis of the post-1970 situation, then the author's treatment of the early postwar relationship is often quite insightful. The chapters on the occupation, San Francisco peace settlement, and the security crisis of 1960 are well written and shed some light on key decisions made by both Japanese and American participants. Some conclusions, however, are drawn hastily, and readers lacking a detailed knowledge of the background will encounter difficulties in certain areas. Whatever one's views on Buckley's assessment of the occupation and the twenty years that followed, criticisms become more serious as the book moves into the present and attempts to recommend a future U.S. course toward Japan.

The nature and future relevance of the Japanese-American alliance is a particularly topical and relevant area of inquiry given the current debate over Japan's global responsibilities. Indeed, the very pa-

rameters of the problem are changing rapidly, as demands for an altered role for Japan in the international arena increase. The analysis served up in this book, however, provides little that is new or startling. Although published in 1992, the book only hints at the existence of Mikhail Gorbachev and contains no mention of the demise of the Soviet Union or that event's implications for the future of the Japanese-American alliance. The central argument contained here is that the post-1945 Japanese-American relationship has been governed by two factors: the American need for a reliable and largely compliant ally in the Pacific, and the Japanese search for new markets and natural resources. The predominant image of Japan as an actor in the international arena is that of passivity; a follower committed solely to the maintenance of the existing economic and political order from which it has benefited so handsomely. Even the marginally more activist image of Japan suggested in some of the data is firmly set in a passive, dependent mold.

Buckley's conclusion that, given its global economic influence, Japan should assume a greater share of responsibility for both its own defense and the maintenance of the global order is neither new nor compelling. Buckley's attempt to illuminate a fascinating and evolving relationship is only partially successful, but it highlights the need for undergraduate texts that are both accessible and stimulating.

MICHAEL A. WEINER
University of Sheffield

DAVID SCHOENBAUM. *The United States and the State of Israel*. New York: Oxford University Press. 1993. Pp. xiv, 404. Cloth \$39.95, paper \$16.95.

David Schoenbaum examines the diplomatic, strategic, cultural, economic, and domestic political factors in the United States and Israel that shaped their relationship since the 1940s. The author suggests that the two states developed a special relationship, although he is aware that tensions and disagreements repeatedly soured the marriage despite its importance to both parties.

In the strategic realm, Schoenbaum emphasizes that the United States unfailingly defended Israeli sovereignty and survival and eventually considered Israel an unofficial ally, but that the development of this partnership was neither smooth nor inevitable. For example, President Dwight D. Eisenhower compelled Israel to evacuate territories occupied during the Suez War. During the "golden age" (p. 183) of the relationship, from 1961 to 1973, the United States provided weapons, shared intelligence, endorsed Israeli retention of the occupied territories, confronted Arab radicals, and rescued Israel by supplying arms during the 1973 war. Thereafter, however, discord surfaced between Washington and Jerusalem. Israel "made the veins throb in American foreheads" (p.

233) by its policy on shuttle diplomacy, the Israeli-Egyptian peace treaty of 1979, the war in Lebanon, and Republican peacemaking plans of 1981–92.

Regarding the political and diplomatic dimensions of the relationship, Schoenbaum probes how the personal views of American and Israeli leaders—such as Ronald Reagan’s “Hollywood poolside Zionis[m]” (p. 273)—affected their diplomacy. The author competently narrates Washington’s involvement in Arab-Israeli wars and peacemaking; his assessment of American “self-deception” (p. 198) in 1973 seems especially persuasive. Schoenbaum even-handedly evaluates the achievements and failures of both states, although many readers will question his suggestion that the Truman administration’s policy toward Palestine “reflected some logic and even consistency” (p. 38). The book shows that the Cold War occasionally influenced Washington and occasionally distracted it from the Middle East.

To his credit, Schoenbaum also examines non-official interaction between the two countries. He explores the relationship between Israel and the American Jewish community, demonstrates how public opinion and domestic policies in both countries affected the binational relationship, and chronicles private philanthropy, investments, and trade. Admirably, Schoenbaum incorporates developments in popular culture, although he might have better elucidated how such factors influenced official policy. Readers will appreciate his engaging, chatty, and anecdotal style.

A few limitations exist in this fine book. Although Schoenbaum bases his survey on an extensive reading of secondary literature and memoirs, he merely dabbles in published U.S. records and virtually neglects available primary sources in American and Israeli archives. This thin documentary base contributes to simple assessments of complicated matters such as the Alpha peace plan. Schoenbaum’s suggestion that American dealings with Egypt in the late 1970s were unprecedented overlooks the pre-1956 view in Washington that the key to Arab-Israeli peace lay in Cairo. Moreover, the important issue of Israeli nuclear capabilities is not examined at length as a factor in the security partnership.

Schoenbaum has entered a crowded field of scholarship on American-Israeli relations. In substance and style, his book is distinct from recent works of William Quandt, Asaf Hussain, and George W. Ball and Douglas B. Ball. Schoenbaum’s study is a valuable synthesis in its own right.

PETER L. HAHN
Ohio State University,
Columbus

THOMAS BORSTELMANN. *Apartheid’s Reluctant Uncle: The United States and Southern Africa in the Early Cold War*. New York: Oxford University Press. 1993. Pp. ix, 298. \$35.00.

Until recently, historians of American foreign relations have generally left Africa to journalists and political scientists. Thomas Borstelmann’s study of the Truman administration and South Africa is an example of a growing interest in Africa as a laboratory to examine America’s Cold War assumptions and the influence of racial considerations on diplomacy.

Borstelmann is delightfully straightforward in his method, sources, and conclusions. His book is a traditional diplomatic history of the Truman administration’s policies toward South Africa with a focus on official policy statements, position papers, and dispatches. His sources are largely State Department records, materials in the Truman Library, and the papers of key officials. He acknowledges that the “archival basis of this study is not multinational. American policy-makers generally occupy center stage in this story, and the research is therefore grounded in American documents” (p. 207).

His conclusions are also direct: U.S. officials recognized the dangers of apartheid and its disregard of basic human rights, but they were unwilling to challenge South Africa as its strategic minerals were crucial to America’s nuclear industry. All other influences on policy “paled in comparison to the ultimate question of uranium ore,” he concludes (p. 129). South Africa’s unwavering commitment to the containment of communism, especially its participation in the Korean War, and encouragement of U.S. trade and investment solidified Washington’s willingness to overlook its racism.

These conclusions are not new, but Borstelmann provides a wealth of detail as to their implementation and implication. The book is especially good at exploring the subtle racism that permeated the State Department and White House. Although few of Truman’s advisors were openly racist, they were products of a segregated society and had little contact with Africans or African Americans. Although willing to challenge segregation at home, they believed in the innate inferiority of the African and saw white South Africans as bastions of stability, anticommunism, Christianity, and economic development on an increasingly unstable and hostile continent.

The book is weakened a bit by its monocausal emphasis on economics and uranium. Although trade with South Africa was crucial to some U.S. industries, it remained insignificant in comparison to other areas. Uranium was important, but America received over 90 percent of its ore from the Belgian Congo, and domestic discoveries in the early 1950s alleviated the dependency on foreign supplies. Perhaps there were more complex reasons for Washington’s reluctance to actively oppose apartheid.

Truman’s decision to maintain normal relations with Pretoria began a pattern that continued through Ronald Reagan’s policy of “constructive engagement.” From 1948 to the mid-1980s, most policy makers argued that the United States should retain economic, political, and cultural contact with South

Africa as sanctions and diplomatic pressure would only further isolate the white minority and lead to even more oppression of the black majority. From Truman on, American leaders were convinced they could moderate the Nationalist government, improve the standard of living of black Africans, and help find a peaceful solution to the complex racial situation in South Africa. They were wrong and their policies served to identify the United States with the racist regime in Pretoria, but their actions were not just the result of uranium or racism; they grew from an arrogant conviction that American economic and political influence could solve any problem, from nation-building in Vietnam to economic development in Latin America to racism in South Africa. It is this element that is missing from this otherwise solid study.

THOMAS J. NOER
Carthage College

CANADA

FRANÇOISE NOËL. *The Christie Seigneuries: Estate Management and Settlement in the Upper Richelieu Valley, 1760–1854*. (Studies on the History of Quebec/Etudes d'histoire du Québec, number 3.) Buffalo, N.Y.: McGill-Queen's University Press. 1992. Pp. xiv, 221. \$39.95.

Lieutenant Colonel Gabriel Christie, a Scottish officer who came to North America in the 48th Regiment, finished the Seven Years' War at Montreal as a deputy quartermaster general. By 1766 he was one of the colony's leading landowners, having bought property from a number of French officers and their families. Françoise Noël focuses particularly on the surveying, settlement, and administration of the lands that became Christie's principal holdings, five as-yet unsettled seigneuries along the strategic Upper Richelieu valley, just north of the U.S. border. Ninety years later, when seigneurial tenure was commuted, the area would have a population of over 20,000.

Although seigneurial tenure has been seen as an increasingly obsolete, development-impeding form, Noël argues that its "relevance . . . in the century after the Conquest . . . stemmed not so much from its differences from freehold tenure as from its similarities" (p. 136). Christie and his heirs, who seldom lived in the seigneuries and spent long periods out of the country, relied greatly on agents. The law gave the seigneur a wide range of rights and possibilities from which a vigilant agent could generate an excellent income as development went forward.

In church-controlled seigneuries, corporate control could ensure a continuity of administration, whereas the "whims of fate" (p. 91) governing human mortality made for discontinuities in private holdings. A change of seigneur could lead to changes in agents, notaries, and their practices, and it often determined who obtained specific mill privileges or timber rights, or where churches and even some villages might be

located. Yet it seems unlikely that the practices of an individual agent or seigneur profoundly affected the timing and essential character of the region's development.

This is a sophisticated study. Still, Noël's phrasings sometimes raise questions about her conceptualization of investment and the development process. For example, she speaks of "windfall profits" (p. 68) instead of return on investment or profits; and of "monopoly" in a number of situations (p. 45) where competition and market forces were clearly at work. Discussing the later years of the system, she uses the politically charged term "burdens" (pp. 120, 135) to include payments that were surely well understood by settlers. Here it is relevant that the Upper Richelieu was "one of the few seigneurial areas to be settled by a significant anglophone population" (p. 52); such migrants had many possible places to settle.

The book is shaped by the character and abundance of the documentation, which includes legal records, administrators' correspondence, and deeds of concession to the land; from the latter, Noël has derived a substantial data base, described in a full appendix. She provides numerous tables and excellent maps, figures, and illustrations, including modern aerial photographs in which the survey and allocation practices described remain clearly visible in the landscape. A few details of the presentation are confusing, however. For example, the large discrepancy in average annual revenue from *cens et rentes* in Bleury and Noyan as given in table 7 and in figure 1 is not recognized, and the reader is left unsure which is the better statement of seigneurial revenues in later years. In table 6, the column of surveyor charges does not add up to the total given.

Knowing how seigneurialism worked is essential to understanding how Quebec's law and society evolved in the first century of the British era. Noël's book is warmly recommended as a meticulous, carefully argued study that should interest not just students of the place and period but all who are interested in the historical relationship between legal forms and actual patterns of social and economic development.

DOUGLAS MCCALLA
Trent University

ROBERT S. ALLEN. *His Majesty's Indian Allies: British Indian Policy in the Defence of Canada, 1774–1815*. Toronto: Dundurn. 1992. Pp. 294. Cloth \$34.99, paper \$19.99.

In British North American colonial history, the period between the American War of Independence (1774–83) and the War of 1812–14, which concluded with the Treaty of Ghent (1815), is an unusual one. Faced with the necessity of protecting Canada against American expansionism at a time when they were caught in the midst of conflicting demands in Europe and various parts of their empire, the British courted

Amerindian alliances as never before or since. It is not surprising that colonial historians have found this period in British-Amerindian relations particularly interesting. It should be pointed out, however, that this surge of British interest in indigenous allies was by no means confined to Canada: in its Asian and African colonies as well, Britain developed a lively appreciation of the war services that the native-born could provide.

Robert S. Allen has a solid record of publications for this period. In his new book, he avails himself of this background to present a detailed picture of Britain's Amerindian policy as it evolved over the four troubled decades that culminated with the ending of the North American colonial wars. Despite Allen's disclaimer to the contrary, this is not a reinterpretation so much as a retelling, albeit one fleshed out with little-known information that reinforces the view that the Amerindian auxiliaries were vital to Canada's defense. In fact, they were the single most important factor. In making use of such allies the British were following in the footsteps of the French, who had long since discovered how effective Amerindian alliances could be in protecting their colonial interests.

Focusing as he does on British policy, Allen does not present as clear a picture of the Amerindian side of the story. For example, it would have been appropriate if, in the appendixes, a text of an Amerindian chief's speech had been included to complement that of Robert Dickson. As for policy, Allen underplays the relationship between the spiritual movement led by Tenskwatawa, the "Shawnee Prophet," and the political and military activities of the prophet's brother, Tecumseh. He does not mention at all the revivalist movement among the Iroquois that became known as the Longhouse religion. It was launched by the Seneca chief Ganiodaio (Handsome Lake), half brother of the war chief Cornplanter (Kaiutwah'ku). The importance of the spiritual factor in guiding Amerindian policy, both in war and in peace, would be difficult to overestimate. On another plane altogether, the book has not been well served by its presentation. Narrow margins and the lack of subheads do not improve the readability of the text.

On the positive side, this book is a closely researched summary of British policy during a critical period in Canada's history, when the European-Amerindian partnership that characterized the fur trade became particularly important in the field of battle. It would be the partnership's swan song.

OLIVE PATRICIA DICKASON
University of Alberta

CARMAN MILLER. *Painting the Map Red: Canada and the South African War, 1899–1902*. (Canadian War Museum Historical Publication, number 28.) Buffalo, N.Y.: McGill-Queen's University Press or Canadian War Museum. 1993. Pp. xvi, 541. \$44.95.

At a time when South Africa is much in the news, it is appropriate that a book should appear that discusses an earlier crisis that contributed greatly to the current situation. Although its apologists defended the South African War as necessary to establish British supremacy in southern Africa, it was evident that the overriding concern was to secure firm control of the gold and diamond fields. Public revulsion at what had been done resulted almost immediately in the grant of self-government to the recently conquered Afrikaners. One unforeseen result of this magnanimity was that, when they embraced the racial doctrine of apartheid in 1948, the Afrikaners controlled more territory and more people than they might otherwise have done. The appalling legacy of these events is apparent today.

The South African War of 1899–1902 is largely forgotten today, and Carman Miller is undoubtedly correct when he suggests that one reason why Canadian historians have tended to ignore or to downplay their country's involvement in it is because they have regarded it as an aberration. Enthusiastic participation in an imperialist war, the aim of which quite clearly was to destroy the independence of a self-governing, albeit not very liberal, minority ethnic group for the obvious benefit of London financial interests, hardly fits into the traditional Whig view of Canadian history as a steady progress toward enlightened, liberal self-government. The fact that it occurred under a Liberal French Canadian prime minister does not help.

In this long-awaited book—the first full-length study of the subject—Miller describes Canadian involvement in the South African War in considerable detail, from the debate over participation to the activities of the men and women in the field. Although not challenging the accepted view that the war sharply divided English and French Canadians and launched the modern French-Canadian nationalist movement, he quite rightly points out that English Canada's response was "less monolithic and selfless" (p. xiii) than historians have tended to claim in the past. He also notes the disturbing familiarity of the debate surrounding the war, comparing it with the larger public controversy over participation in World War I that nearly destroyed the Canadian federation, although he does not develop this theme.

This is an excellent, comprehensive history of Canadian involvement in the South African War and the enormous impact it had on the country. Aside from its length, the result of what some might regard as excessive detail, the organization of the book contributes to the sense that Miller became somewhat overwhelmed by the magnitude and scope of his research, with the home front being relegated to a separate chapter and some repetition appearing in accounts of regimental activities. Nevertheless, he has written a highly readable book that will be recognized as the definitive study for many years to come. The fine maps and thorough bibliography are invaluable as

well. All students of imperialism, and particularly of the history of Canadian foreign policy, will long remain in his debt.

BRIAN DOUGLAS TENNYSON
University College of Cape Breton

GERALD TULCHINSKY. *Taking Root: The Origins of the Canadian Jewish Community*. (The Brandeis Series in American Jewish History, Culture, and Life.) Haver, N.H.: University Press of New England. 1993. Pp. xxvi, 341. \$35.00.

Until recently, Canadian-Jewish history was largely the preserve of amateurs and antiquarians, a phenomenon partially explicable by the small size and slow growth of the community. Although the fifth largest Jewry today, in 1900, some 150 years after the first Jews arrived in Canada, the community was smaller than that of Belgium. Just before the Holocaust, two decades after this book's closing date, it numbered only about 150,000 members. Because of their late arrival, but also because Canada lacked a sociopolitical ethos for incorporating people who were neither French and Catholic nor British and Protestant, most Canadian Jews were still outsiders. The community had produced few notables and was often perceived as an appendage of American Jewry.

The postwar decades witnessed rapid communal growth and the acclimatization of Jews in an increasingly pluralistic and nonisolationist Canada. As the community matured, it began to attract the attention of professional historians such as Gerald Tulchinsky. Ironically, many of the professionals, including Tulchinsky and myself, have focused on the earlier era, when the community was "taking root." No one has yet attempted a synthesis like Howard Sachar's magisterial *History of the Jews in America* (1992).

This is not to take away from the significant achievements of the volume at hand. A social and business historian, Tulchinsky is particularly strong in these areas. He breaks new ground in using business records as a source for Canadian-Jewish history. He shows extraordinary understanding of the Jewish labor movement in the early twentieth century, especially of the conflicts between Jewish bosses and Jewish laborers in Montreal. Tulchinsky may be the first to remark the apparent absence of social consciousness among Montreal's rabbis during those years. He could, however, provide a fuller portrait of business people. Their cultural, philanthropic, political, and religious interests often go all but unmentioned.

In general, this book is culturally undernourished. There are meaty chapters on Zionism and on Montreal's De Sola family (Tulchinsky is the first to make extensive use of the correspondence between Abraham De Sola and Isaac Leeser of Philadelphia). Religion, however, is treated like icing on the cake rather than the main course of Jewish existence.

Much more should be said about the traditionalism of Canadian Jews (often perfunctory) and the influence (often subtle) of American religious movements.

Tulchinsky's focus on the Canadian context results in important new insights: that the Canadian government's interest in Jewish settlers in 1881-82 was prompted by a desire to attract Rothschild investments, for example. It also results in the understating of American influence and in other problems. That the Workmen's Circle, the National Council of Jewish Women, and various Jewish charitable federations were American imports goes unremarked here. That businesses attractive to American, British, and other Jews, like merchant banking and department stores, were virtually *judenrein* in Canada is not explained. More astonishing is Tulchinsky's rather apologetic evaluation of French-Canadian anti-Semitism. It is untrue, for instance, that Montreal's daily, *Le Devoir*, was "not spreading antisemitism among its readers" (p. 243) after 1910, although its reporting was sometimes balanced.

Despite some shortcomings, this is a most important book, offering new perceptions of some of the main issues of Canadian-Jewish history and rehearsing well still-acceptable traditional views. It may be hoped that Tulchinsky or someone of equal skill and professionalism will now undertake an overview of what has become in the last half century one of the world's largest and most interesting Jewish communities.

MICHAEL BROWN
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JOHN ENGLISH. *The Worldly Years: The Life of Lester Pearson*. Volume 2, 1949-1972. New York: Alfred A. Knopf. 1992. Pp. ix, 473. \$32.50.

This volume completes John English's biography of Lester Pearson. It achieves the superb quality of the earlier *Shadow of Heaven* (1989), which won several awards. The new work centers on Pearson's life when he served as minister of External Affairs, Liberal Party leader, and prime minister. English continues to base his solid research on Pearson's personal papers, a wide range of the private papers and diaries of Pearson's contemporaries, and many personal interviews. The result is a picture of the public and private Pearson as well as a perceptive analysis of the events under discussion.

Pearson's maturation occurred during the nationalist excesses of the 1930s and the cooperative achievements of the 1940s. These factors, together with his belief that liberalism was the philosophy most able to contribute to the welfare of the individual, meant that Pearson grew to appreciate the fallacies of nationalism and the value of internationalism. He thus perceived the creation and functioning of international bodies such as the North Atlantic Treaty

Organization and the United Nations as vital to world peace. His Eurocentrism was his one major fault at this level. Canada and Britain were his two favorite countries, but he had real difficulties in appreciating Asian points of view. At the international level his greatest accomplishment was his involvement with the creation of the first U.N. peacekeeping force to defuse the Suez crisis. The Nobel Peace Prize was his reward.

On the national level, Pearson's work also demonstrated flexibility of perspective and determination of effort. If changing world conditions required a shift from a nationalist to an internationalist stance, then in Canada Pearson would move from a strong centralism to an appreciation of the concept of limited identities. For Pearson, meeting the needs of French Canada, which was coming out of its "quiet revolution," was the key to national success. More generally, welfare capitalism created the economic base for wide-ranging achievements: the Canada Pension Plan and Canada Assistance Plan, the revision of fiscal federalism, the creation of the Economic Council of Canada, youth allowances, student loans, more regional development funds, expanded funding for technical and vocational training, the first steps toward national medicare, a more liberal immigration policy, unification of the armed forces, the Canada-United States auto pact, expansion of the concepts of bilingualism and biculturalism, and the maple-leaf flag, which Pearson considered his most prized achievement.

All of this was achieved through a revolution in government. Instead of having decisions emanate from a central ministry and bureaucracy, Pearson initiated a process of federal/provincial diplomacy. There had been only seven federal/provincial conferences in 1939, but there were 125 in 1965, with as many as 200 participants at a typical conference.

Pearson was the only Canadian prime minister to be educated as a historian. He understood history to be generated, as English quotes him saying, by "people; yes, to one person and his own response to the challenges that confront him" (p. 145). The Nobel Peace Prize signifies his success at the international level at generating history. At the national level success was not so dazzling, yet it was equally significant for the long-term success of Canadian society.

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LATIN AMERICA

LESLIE G. DESMANGLES. *The Faces of the Gods: Vodou and Roman Catholicism in Haiti*. Chapel Hill: University of North Carolina Press. 1992. Pp. xiii, 218. Cloth \$32.50, paper \$12.95.

This book's title suggests that its major theme is the relationship between Vodou and Roman Catholicism, two religious systems that are inextricable elements of

Haitian history, culture, and politics. It is predominantly about Vodou, however, and Leslie G. Desmangles discusses Roman Catholicism only to show how elements of that church's discipline, liturgy, and hagiology acted both as masks for the preservation and as catalysts in the creolization of rituals and cosmologies that derive from various West African sources.

Desmangles specifies three main goals: to clarify how historical events have shaped Vodou; to describe cultural continuity and change in Vodou in terms of Vodou's development; and to describe what is specifically creole about Vodou as distinct from those elements that are continuous with African, European, and Amerindian (Taino) origins.

The thesis of the book is the idea of "symbiosis" as the explanation of the relationship of the African and European Catholic traditions in Vodou. Desmangles's intent is to counter the idea of the fusion of these traditions implied in the accepted notion of syncretism. He disassociates the term "symbiosis" from its biological meaning to apply it in an ethnological sense, according to which the diverse religious traditions of two continents are juxtaposed in space and time without fusion to constitute the whole phenomenon of Vodou. The concept at first seems strained, and its advantage over carefully defined uses of more commonly recognized terms such as syncretism or creolization is not immediately obvious. But Desmangles patiently demonstrates throughout the book the aptness of the idea, and I was convinced of its precision by the end.

By the end of the book Desmangles also has unfolded the complex system of ritual, myth, and cosmology that constitutes Vodou. Much of the information in the book can be found throughout the writings of earlier scholars, such as Jean Price-Mars, Maya Deren, Alfred Metraux, Harold Courlander, and Melville Herskovits, all of whom Desmangles cites respectfully. Indeed, this book will contribute to the renaissance interest in Herskovits's ideas on continuity and change in African culture in the Americas. Desmangles's accomplishment is to synthesize earlier research, fortify it with his own ample fieldwork in Haiti and Africa, and unify the material with his concept of symbiosis. The result is a well-illustrated and engrossingly written handbook on Vodou.

Desmangles provides linguistic authenticity by adopting Yves Dejean's system of Creole orthography, used in Haitian schools since 1986, in preference to the francophone orthography most widely used by earlier scholars. Thus, in the Dejean system, "Bon Dieu" becomes "Bondye" (the all-powerful deity who heads all Vodou pantheons), "hounfort" becomes "ounfo" (a Vodou temple), the "loas" become the "lwass" (the deities), and so on, so that an anglophone reader can "hear" the Vodou (earlier "voodoo," "voodoo," "vaudou," and so on) terms more accurately. The key terms discussed in the text are also defined in a useful glossary.

As a student of religions, I found Desmangles's book most rewarding. As a historian, I am less than satisfied with his survey of Vodou's development in relation to Haiti's political and ecclesiastical history, which seems rather sparsely chronicled after his introductory proposal of historical explanation of Vodou as a major goal. And I would have welcomed an attempt to analyze the relationship between Vodou and the liberationist Catholicism represented by Jean-Bertrand Aristide, a relationship that Desmangles suggests but does not discuss.

These are minor reservations, however, in light of this book's overall worth. Desmangles's accessible study goes far to counter the terrified consciousness that underlies to some extent most of the literature about Haiti that comes to the attention of North American readers. By giving only half a page of text and one discursive endnote to zombification, for example, Desmangles puts that phenomenon into perspective within the theological diversity and cosmological richness of Vodou.

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TERESITA MARTÍNEZ-VERGNE. *Capitalism in Colonial Puerto Rico: Central San Vicente in the Late Nineteenth Century*. (University of Florida Social Sciences Monographs, number 78.) Gainesville: University Press of Florida. 1992. Pp. xiv, 189. \$27.95.

This book by Teresita Martínez-Vergne is a case study of a central factory during the technological revolution that transformed the Caribbean sugar industry in the years around 1900. San Vicente was Puerto Rico's first factory. It had a brief career, passing quickly from its opening in 1873 to bankruptcy in 1879. There followed various attempts at financial resuscitation until final dissolution in 1895. The theme of the book is to explain this failure, and the author forewarns that it "resulted ultimately from the clash between the owner's ambitious plans and the context in which he launched the novel project" (p. ix). The first two chapters of the book contain a description of this context, a review of the ideas of Puerto Rican agricultural reformers and a discussion of the "concept of centralization." The three chapters on San Vicente itself emphasize the accumulation of land, labor relations, and the difficulty of raising capital. There is a brief conclusion.

The author uses the experience of San Vicente to question the wisdom of the reformers' support of centralization, to add to the literature on capitalism in rural Latin America, and to address debates in Puerto Rican historiography.

My difficulty with the book lies in resolving where the author stands on certain important issues. The problem begins in the preface with defining the capitalist nature of San Vicente. She writes that centralization "did not result in the establishment of

capitalist relations of production . . . [and] did not produce the types of class conflict associated with capitalism" (p. xii). But it was nevertheless a "bona fide example of an attempt to establish capitalist relations of production" (p. xiii). This provides the basis for her claim that her work "challenges interpretations that identify the arrival of capitalist relations of production with the establishment of large U.S. corporations" (p. xiii). There may not have been "class conflict" in her own or in Marxist terms, but the discussion of labor relations in chapter 5 shows that there was indeed bargaining between owner and employees, and there was the resolution of conflicts of interest, albeit in a different form than unionization and strikes. She is ambivalent, too, about the significance of landownership. She reproaches the owner of San Vicente for acquiring a large amount of land (pp. 39, 76, 86–87) when in her view he should have concentrated his resources on the factory itself. Yet she recounts the several advantages of landownership for recruiting and retaining labor (pp. 75–76, 85, 103–04, 109) and for controlling the supply of cane (p. 85). Factory owners without land had to contract with neighbors for cane, but her paragraph on this topic (p. 76) oversimplifies the relations between factory owner and cane grower. Contrary to her claims, the contract she advocates here was surely unlikely to protect the interests of a factory owner in a period of declining sugar prices because at some point the growers would have stopped growing cane, leaving the factory owner with an idle plant.

If indeed the San Vicente experiment was premature, and its failure not in good part the fault of its owner, what measures should the sugar planters have adopted to revitalize their industry? The author is uncertain. Improvements in the banking and the transportation systems of Puerto Rico no doubt would have been helpful, but she does not have an alternative to centralization.

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JAMES LOCKHART. *Nahuas and Spaniards: Postconquest Central Mexican History and Philosophy*. (Nahuatl Studies Series, number 3; UCLA Latin American Studies, number 76.) Stanford: Stanford University Press or UCLA Latin American Center Publications, Los Angeles. 1991. Pp. xiii, 304. Cloth \$42.50, paper \$18.95.

JAMES LOCKHART. *The Nahuas after the Conquest: A Social and Cultural History of the Indians of Central Mexico, Sixteenth through Eighteenth Centuries*. Stanford: Stanford University Press. 1992. Pp. xv, 650. \$60.00.

These two books by James Lockhart, especially the impressive *Nahuas after the Conquest*, offer the fruit of nearly two decades of sustained scholarly work on the history of colonial Mexico, although it seems fair to say that the eighteenth century has been somewhat stunted by Lockhart. Arguably the only progenitor of

a recognizable "school" of colonialist scholarship in recent decades, Lockhart has opened to his readers in these studies, and in a series of essays and skillfully glossed translations, the riches of post-conquest Nahuatl (Aztec native-language) documents, and through them a stunning vista of cultural change in central Mexico. Comparisons will inevitably be made with Charles Gibson's *Aztecs under Spanish Rule* (1964), from which Lockhart has in part drawn inspiration, but the relationship is finally one of considerable complementarity, even if Lockhart's study in some ways marks a considerable technical and interpretive advance over the older work.

The essays in *Nahuas and Spaniards*, most published previously, constitute, in the author's words, "a companion volume" (p. xi) to the larger work, in which the themes and sources are very similar. Nonetheless, this shorter volume is by no means merely a technical appendix but rather explores aspects of historiography and of Nahua (that is, Aztec) and native-Spanish social life alluded to in *The Nahuas after the Conquest*. Among the most interesting of these essays is a masterful appreciation of Gibson's work in which Lockhart finds his own later emphasis on the substantial post-conquest survival of Nahua city-state (*altepetl*) organization foreshadowed, although he perhaps tends to undervalue other aspects of Gibson's treatment because of his own scholarly preoccupations. Also extremely enlightening are an essay on the elements of Nahua local and corporate identity in the late-colonial period and an essay on the interactions of Spaniards and Indians in the Toluca Valley in the sixteenth century. Both pieces resonate with themes treated in a somewhat more abstract and language-centered manner in the larger study, which echoes the descriptive social profiling familiar from the author's earlier work on colonial Peru.

In *The Nahuas after the Conquest*, Lockhart has created an instant classic of the ethnohistorical and broader colonial literature. Its encyclopedic breadth—touching on everything from language phenomena to landholding patterns, and from kinship structures to material life—and its deep, reverential empiricism almost bludgeon the reader into an awesome respect for the author's masterful scholarship. Indeed, despite the generally clear and engaging writing, the seriousness of the book and the sheer amount of detailed philological analysis (Lockhart has referred to his methodological technique and that of his students as a "New Philology") make certain parts of it rather heavy going. Yet Lockhart's keen understanding of the myriad sources that he commands with consummate technical expertise—Spanish-directed native-language ethnographic accounts, Nahua historical chronicles, testaments, litigation records, literary texts, and many other sorts of primary sources, almost exclusively in Nahuatl—shines forth on every page. The general movement of the work is cognitively inward, through a series of eight core chapters that range from a brilliant exploration of Nahua

political organization at the *altepetl* and household levels, to social, economic, and religious life, and thence to language phenomena and forms of cultural expression.

Among Lockhart's most important findings, sure to provide an interpretive principle for post-Lockhart ethnohistorical studies, is the pervasively cyclical and modular (or cellular) quality of Nahua culture, an argument he makes most tellingly in writing of the essentially composite rather than hierarchical internal organization of political entities at all levels. Lockhart reiterates also his by now well-known theory of periodization in post-conquest Nahua cultural change, which involves an era of natives' circumscribed adoption of Spanish cultural practices (1521–50), a second stage of rapid adoption of further traits (1550–1650), and a third stage lasting through the colonial period (1650–1821), in which continued accommodation occurred at the deeper levels of indigenous society.

Readers of a work as complex and ambitious as *The Nahuas after the Conquest* will inevitably find aspects of it to criticize. Indeed, the language-centered nature of the project itself must raise some criticisms. Although Lockhart deals admirably with language change over time, he does not seem to allow for the instability of meaning in his Nahua sources themselves, particularly for language and linguistic usage as reflexes of power and class relationships. Furthermore, one often has the feeling that when he speaks of non-linguistic behaviors—patterns of landholding, for example—he is describing language about these phenomena rather than the phenomena themselves. Also, Lockhart's emphasis on the relatively smooth transitions from one of his great periods to another, and his deemphasizing of political or cultural conflict, may in large measure be a consensual artifact of the very process of writing Nahua documents, and of the people who took to this practice and later to the use of Spanish. Finally, Lockhart's exclusion from his text of theoretical work from other disciplines on important aspects of his themes may frustrate some readers. Can Jack Goody have nothing interesting to say on the introduction of Western literate traditions, or Hayden White on the relationship of annals to other forms of historical production? Nonetheless, these are all debatable points of method and theory, and Lockhart's implicit stance on them is principled rather than taken by indirection. On the whole he has produced in *The Nahuas after the Conquest* not only a work of mature scholarship but also a landmark study of cultural change in the early modern period.

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BARRY GOUGH. *The Falkland Islands/Malvinas: The Contest for Empire in the South Atlantic*. Atlantic Highlands, N.J.: Athlone. 1992. Pp. xvi, 212. \$65.00.

This is a flawed book. Barry Gough provides sound insights into the Falklands conflict from a British perspective. But the author's knowledge of the Argentine context is weak.

The British settled the islands in 1765. They learned unhappily that a French settlement had taken root a year before. France had honored Spain's claim and departed. The British refused to leave until Spanish authorities in Buenos Aires cleared them out in 1770. Following their expulsion, Spain allowed the British to return ceremonially and remain for two years, at the end of which Britain departed.

Argentina took formal possession of the islands in 1820. Gough portrays Louis Vernet as an enlightened governor. The crucial event, portrayed vividly by the author, occurred when Vernet jailed the crews of three U.S. ships that had violated Vernet's regulation of the whaling and sealing industries. But hot-tempered U.S. captain Silas Duncan ransacked and burned Puerto Soledad in December 1830. Equally important, Duncan declared the islands "free of government." Gough points out that the British Admiralty recommended reoccupation of the Falklands when Argentine leader Juan Rosas could not obtain U.S. agreement for reparations involving Duncan's savage attack.

The critical question of sovereignty is not examined thoroughly. Clearly, the subsequent British reoccupation surprised the Argentines, who maintained that Buenos Aires inherited sovereignty as a successor state. Britain insisted consistently that the withdrawal in 1774 did not weaken British claims to sovereignty. But the author cannot make up his mind. At various points Gough insists that Argentine claims are "substantial" (p. 138), "flimsy," (p. 140), and "irrefutable" (p. 155).

Moreover, Gough does not understand relations between Rosas and Britain. Gough misinterprets the British invasion of Buenos Aires in 1806, when local militia, not Spanish troops, forced the British out. Rosas was not "indifferent" (p. 44) to British trade; he attempted to establish a federalist tariff policy to protect local markets and industries. Nor was Rosas a "director" (p. 45).

Gough's review of the process of empire-building provides a theoretical framework within which one can understand the dynamics of South Atlantic politics that resulted in the 1982 war. The strong feature of this study is its emphasis on British commercial desires. Gough maintains convincingly that British trade and naval interests went hand in hand. He demonstrates that the Falklands eventually became profitable. Quite succinctly, he sees a steady continuity in post-1840 events. Gough also demonstrates that Argentina took up the Malvinas issue in 1884 and dogged the British for fifteen years. Here is additional proof that the Argentine elite were not submissive lap dogs. It is interesting to learn that Britain expanded its imperial claim to the region in 1917.

Overall, this is a modest contribution to the litera-

ture. There is little concerning the 1982 war. The author's claim to have provided an objective view of British and Argentine claims does not stand up to close scrutiny. The bibliography is fine, but all the primary sources are in English. Reverence characterizes Gough's assessment of Britain. And he simply ignores many of the contentious issues raised in his secondary works. The author also has a tendency to stray off the topic, particularly in the first fifty pages. For such a short book, Gough should have been more circumspect.

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HERNÁN HORNA. *Transport Modernization and Entrepreneurship in Nineteenth Century Colombia: Cisneros and Friends*. (Studia Historica Upsaliensia, number 172.) Uppsala, Sweden: Acta Universitatis Upsaliensis. 1992. Pp. 189. 182 KR.

This is an important biography of Francisco Javier Cisneros (1836–1898), an upper-class Cuban revolutionary and entrepreneur who built railroads in Cuba, Peru, and Colombia. The main focus of this book is Cisneros's career in Colombia, presumably where he had his greatest impact. Unfortunately, details of his life elsewhere are overly abbreviated. Nevertheless, Hernán Horna successfully offers Cisneros's career as an interesting case study from which to view entrepreneurship, technological change, modernization, and politics in a Latin American context with a special insight into Colombian history.

Born into an elite Creole family and educated in railroad engineering at the University of Havana and Troy Polytechnic Instituté (New York), Cisneros was well established, rich, a respected writer and editor, and director of the Ferrocarril del Oeste by the age of thirty-two. He lost his wealth and standing with his involvement (1868–73) in the Cuban revolutionary movement. He emigrated to the United States in 1870 and became a U.S. citizen in 1874. As the business and logistics manager of the Cuban revolutionary movement abroad, he traveled to Colombia in 1870 and to Peru in 1871 recruiting men and raising funds, but he ceased active participation in 1873. Scrambling to provide for himself and his family, Cisneros organized other Cuban technicians into a New York engineering firm and peddled their expertise in the construction of Andean railroads. In 1873 he contracted to build a railroad in Antioquia, Colombia.

His Cuban experience convinced him that narrow-gauge railroads were the appropriate technology for Colombia. Given their lower cost and Colombia's lack of wealth and slow export expansion, he was probably right. Cisneros trained a generation of Colombian engineers, and they faithfully followed his manuals in constructing Colombia's railroads.

Between 1874 and 1885 Cisneros and his team of engineers contracted to build Colombia's most important lines, but they only finished fifteen of the 201 kilometers from the Magdalena River to Medellín, thirty-one of the 138 kilometers from Buenaventura to Cali, twenty-three of the forty-six kilometers from Honda to La Dorada, and thirty-one of the 111 from Girardot to Bogotá. Nevertheless, Cisneros was always able to finesse a settlement with the departmental and national governments and other participants and to move on to other projects without going broke or alienating too many Colombians. What was completed bridged major bottlenecks and eased trade with the outside world, and Cisneros had key political and financial support. Given the limitations of Colombia's geography and politics and the scarce profits in Colombian railroads, this was an incredible tribute to Cisneros's abilities.

Typical was Cisneros's purchase in 1884 of the fifteen-kilometer line linking Barranquilla and the Atlantic Ocean. By 1893 he had modernized its rolling stock and extended the line to thirty-three kilometers with the third largest iron dock in the world, effectively eliminating lighter traffic. He rechartered this as a joint-stock company in England in a move designed to provide English diplomatic protection against the interference of the Colombian government.

Political historians of Colombia would be well advised to look at Horna's book for some of the political fault lines produced by railroad construction. Sadly missing, however, are meaningful comparisons with studies of railroad-building elsewhere and workable measures of how railroads lowered transportation costs and modernized Colombia. Nevertheless, as entrepreneurial history the book merits a thorough reading.

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MARTIN EDWIN ANDERSEN. *Dossier Secreto: Argentina's Desaparecidos and the Myth of the "Dirty War."* Boulder, Colo.: Westview. 1993. Pp. xii, 412. Cloth \$59.00, paper \$17.95.

Martin Edwin Andersen argues that the Argentine military's "dirty war" was not a war but a witch hunt. His reporting of events behind the scenes, intended to ensure that the military's fictionalized account of the "disappeared" will never again replace fact, is an invaluable source of information concerning Argentina's recent and fitful history.

In witch hunts, however, the victims do not take the initiative of attacking military garrisons after building war chests through bank expropriations and kidnappings of corporate executives; and wars are not defined by the number of shots exchanged or by the

size of the contestants, but instead by armed struggles involving ethnic groups, churches, religious sects, and political parties, not just regular armies. Although in Argentina the bulk of the "disappeared" were victims of a witch hunt, the guerrillas waged a miniwar against the military over a ten-year period from 1969 to 1979. The "dirty war" was the military's response to this challenge. It was no "myth" that guerrillas might spark a civil war, such as happened later in Nicaragua and El Salvador. Nor was the "dirty war" launched after the guerrillas had been routed. The most important military engagements had yet to come, the assault on the army's garrisons in Formosa and Buenos Aires in October and December more than six months after the "dirty war" began in February 1975.

The book is further marred by Andersen's tacit acceptance of the "Two Devils Thesis." He shows little understanding of, much less sympathy for, the "legitimacy" of the military's systematic repression of international subversion on Argentine soil, even if the "dirty war" was not an opening skirmish of World War III. As for the guerrillas, he writes them off as the phony pied pipers of revolution who led a generation of well-meaning students and workers to their deaths for a hopeless and questionable cause. Although their ends placed them at loggerheads, the guerrillas and the military shared what Andersen perceives to be the same diabolical premise: the end justifies the means.

Ironically, the book's leading villain is not a mass murderer but a guerrilla leader, the top Montonero and public enemy number one during the military process. On the basis of flimsy circumstantial evidence controverted even before the book appeared, Mario Firmenich is portrayed as a military intelligence agent who betrayed his comrades, as a despicable being even worse than Jorge Videla, the leading military general behind the repression. As prime evidence for Firmenich's role as a double agent, Andersen relies on interviews with a mysterious "Sam," a U.S. intelligence official whose real identity is kept secret, testimony that would not hold up in a court of law. A key exhibit in the case against Firmenich was the Montonro press conference on June 20, 1975, concerning the \$60 million ransom that was a record for extorsive kidnappings. The press conference took place in a safe-house used by security forces to torture illegally detained leftist suspects, from which Andersen concludes that Firmenich was linked to government intelligence agents. But the Montoneros were unaware of the sinister uses of the house they had rented for their alleged "fiesta," as another investigative reporter, Juan Gasparini, has explained (*Montoneros* [1988], cited by Andersen, pp. 78–79 n.). In part, then, Andersen replaces one fictionalized account of the "dirty war" with another.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

GENERAL

WOLFGANG KÜTTLER *et al.*, editors. *Geschichtsdiskurs. Volume 1, Grundlagen und Methoden der Historiographiegeschichte*. (Fischer Wissenschaft.) Frankfurt a.M.: Fischer Taschenbuch. 1993. Pp. 375. DM 24.90.

JÖRN RÜSEN, "Moderne" und "Postmoderne" als Gesichtspunkte einer Geschichte der modernen Geschichtswissenschaft. LUTZ NIETHAMMER, Die postmoderne Herausforderung: Geschichte als Gedächtnis im Zeitalter der Wissenschaft. WOLFGANG KÜTTLER, Erkenntnis und Form: Zu den Entwicklungsgrundlagen der modernen Historiographie. FRANK R. ANKERSMIT, Historismus, Postmoderne und Historiographie. CHANG-TZE HU, Modernität der Historie in China und historische Identitätskrise. ERNST SCHULIN, Vorbemerkung zum Periodisierungsproblem. LUTZ RAPHAEL, Epochen der französischen Geschichtsschreibung. HANS SCHLEIER, Epochen der deutschen Geschichtsschreibung seit der Mitte des 18. Jahrhunderts. JÜRGEN OSTERHAMMEL, Epochen der britischen Geschichtsschreibung. HORST WALTER BLANKE, Typen und Funktionen der Historiographiegeschichte: Eine Bilanz und ein Forschungsprogramm. IRMGARD WAGNER, Geschichte als Text: Zur Tropologie Hayden Whites. HERTA NAGL-DOCEKAL, Für eine geschlechtergeschichtliche Perspektivierung der Historiographiegeschichte. RÜDIGER VOM BRUCH, Historiographiegeschichte als Sozialgeschichte: Geschichtswissenschaft und Gesellschaftswissenschaft. WOLFGANG KROHN, Die Wissenschaftsgeschichte in der Wissenschaft: Zu einer Historiographie der Wissenschaftsgeschichtsschreibung. RALF POSSEKEL, Rehabilitation des Widerspruchs: Plädoyer für eine formale Geschichtsphilosophie. WILFRIED NIPPEL, "Geschichte" und "Altertümer": Zur Periodisierung in der Althistorie. DIETER BERG, Mediävistik—eine "politische Wissenschaft": Grundprobleme und Entwicklungstendenzen der deutschen mediävistischen Wissenschaftsgeschichte im 19. und 20. Jahrhundert. GEORG G. IGERS, Das Programm einer Strukturgeschichte des historischen Denkens: Anmerkungen zu H. W. Blanke. HANS-PETER JAECK, Die Erklärung historiographischer Texte als Ausgangspunkt einer "Strategie" der Historiographiegeschichte. HANS-JÜRGEN PANDEL, Wer ist ein Historiker?: Forschung und Lehre als Bestimmungsfaktoren in der Geschichtswissenschaft des 19. Jahrhunderts. HANS-JÜRGEN LÜSEBRINK, Tropologie, Nar-

rativik, Diskurssemantik: Hayden White aus literaturwissenschaftlicher Sicht. ERNST SCHULIN, Nach der Postmoderne.

PATRICIA COOK, editor. *Philosophical Imagination and Cultural Memory: Appropriating Historical Traditions*. Durham, N.C.: Duke University Press. 1993. Pp. vi, 243. Cloth \$39.95, paper \$15.95.

GEORGE ALLAN, Traditions and Transitions. DONALD PHILLIP VERENE, Two Sources of Philosophical Memory: Vico versus Hegel. ALASDAIR MACINTYRE, Are Philosophical Problems Insoluble? The Relevance of System and History. J. B. SCHNEEWIND, Modern Moral Philosophy: From Beginning to End? GEORGE R. LUCAS, JR., Refutation, Narrative, and Engagement: Three Conceptions of the History of Philosophy. ARTHUR C. DANTO, The Shape of Artistic Pasts: East and West. LYNN S. JOY, Humanism and the Problem of Traditions in Seventeenth-Century Natural Philosophy. ROBERT CUMMINGS NEVILLE, The Symbiotic Relation of Philosophy and Theology. EVA T. H. BRANN, The Six Silences of a Grecian Urn. GEORGE L. KLINE, Changing Russian Assessments of Spinoza and Their German Sources, 1796–1862. JOHN S. RICKARD, Tradition and Intertextual Memory in James Joyce's *Ulysses*. STANLEY ROSEN, Plato's Quarrel with the Poets.

JOHN BREWER and ROY PORTER, editors. *Consumption and the World of Goods*. New York: Routledge. 1993. Pp. xix, 564. \$59.95.

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ARTICLES

TO THE EDITOR:

For long I have admired the *AHR*'s willingness to recognize dissenting opinions, however controversial these may be; I hope you will see fit to recognize mine.

It is certainly not my intention to initiate a debate, least of all in a field about which I know less than the average student of history. Please allow me, nevertheless, a brief comment on David Eltis's carefully researched, painstakingly argued article "Europeans and the Rise and Fall of African Slavery in the Americas: An Interpretation" (*AHR*, 98 [December 1993]: 1399–1423).

I found it a bit surprising that, while the essay carefully parries possible objections, the European-dominated world market along the Atlantic described by Immanuel Wallerstein, Fernand Braudel, R. Davis, J. H. Parry, and others does not earn so much as a nod in a footnote. Eric Williams is mentioned briefly and is made to agree with Eltis.

More shocking to me was what appears to be a central argument: "the inability of the European to think in terms of slavery for other Europeans" (p. 1414) and the implication of that thesis, that Europeans were racist from the start. A visit to the Holocaust Museum in Washington, D.C., reminded me of something your readers, and most of your authors, already know: that, in addition to the mass murder of other Europeans, Europeans did not hesitate to enslave millions of Europeans, whether it be in the framework of the Todt Organization (which some might dismiss as merely forced labor) or beyond any organization, as slaves pure and simple, unto death.

This example of Europeans enslaving other Euro-

peans is taken not from the eighteenth century, or even from the early modern centuries, but from our own.

MARIO D. FENYO
Bowie State University

DAVID ELTIS REPLIES:

I thank Mario Fenyo for his comments on my article. In response, let me say that the absence from my footnotes of some major contributors to the historiography of European expansion was due entirely to the constraints of space. On Eric Williams, I must disagree with your correspondent. *Capitalism and Slavery* argues that slavery in the Americas came about without perceived racial differences between African and European playing a major role. There is nothing in my essay that supports this position, and I am unclear why Fenyo thinks that "Eric Williams . . . is made to agree with Eltis."

The enslavement and destruction of Europeans by Europeans in the twentieth century would seem to be a different issue from the one addressed in my work. There was clearly an economic component in the Nazi forced-labor program. However, if the *primary* motivation *was* economic, then the Nazis certainly got it wrong. Working individuals to death or killing them outright was scarcely consistent with extracting the maximum amount of labor. I am certain that British, French, and antebellum U.S. slave owners would have gotten more out of six million Jews (as well as gypsies and Slavonic peoples) had they obtained them as slaves.

In short, I think the twentieth century lends support to my view that the systematic degradation of another (and all that other's progeny) into full chattel slavery for the purpose of maximizing profits was something that Europeans could envisage only for non-Europeans before the mid-nineteenth century, and for no one at all thereafter. For all their barbarities, twentieth-century Europeans could no more bring themselves to convert their fellows into full chattel slaves than could their seventeenth-century predecessors. The buying and selling of individuals on open markets, and the systematic use of such individuals for the financial gain of their owners, has

not happened in twentieth-century Europe. It is thus hard to see in the twentieth century a counterpart to African slavery in the Americas.

DAVID ELTIS
Queen's University
Kingston, Canada

REVIEWS OF BOOKS

TO THE EDITOR:

In his review of my book, *German Radicals Confront the Common People* (AHR, 98 [December 1993]: 1627), Jonathan Sperber accuses me of having been tendentious, of having imposed my own conservative political views on the empirical evidence. He does acknowledge, however, that my argument for a more cautious approach to democratic traditions, and especially for a better understanding of the persons behind political epithets, is supported "with a mass of archival evidence from judicial records." Indeed, the evidence exists. It has always existed. It is available in quantities of a kind making any presumed fiddling for desired effect entirely redundant. Sperber conveniently ignores my lengthy opening "Note on Sources" that identifies the problems associated with interpreting judicial records and the method employed in the research for countering bias. His quarrel with me appears to be based primarily on our conflicting interpretations of the depositions recorded for the *Oberlandesgericht* Zweibrücken for the 1849 Palatine uprising. The objections then become the basis for discrediting the book as a whole.

Perhaps two examples from those files, drawn from his review, will serve to illustrate the nature of the problem that divides us. Sperber is upset with the description I provide for the Bergzabern democrat, Valentin Borscht—a "sinister figure, gaunt and swarthy, black-haired and black-bearded" (*German Radicals*, p. 242). I make "nasty remarks" and engage in "name-calling," he writes. But my account of this man's character and behavior in 1849 is drawn from the depositions of five witnesses (p. 243, notes 28 and 29). Sperber's account of Borscht's democratic proclivities is based on the testimony of one witness (*Rhineland Radicals: The Democratic Movement and the Revolution of 1848–1849*, 267). I fail to see why my five should be less credible than his one. The reference in the review to "a town fool . . . utterly pathetic figure" is to Heinrich Antes, a revolutionary in Wald-fischbach. My account is based on the depositions of

three witnesses. Antes is not included in *Rhineland Radicals*. I am accused of "repeatedly crossing the line separating an objective work of scholarship from a political tract," while Sperber poses as a pillar of objectivity. Interesting. My comment, used against me in the review as a "nasty remark," that participants in mass meetings had "time on their hands and mischief in their hearts" (*German Radicals*, p. 301), was intended to illustrate the fact that "mass meetings were indeed festivities," which was "a large part of their attraction" (*Rhineland Radicals*, p. 220).

In *Rhineland Radicals*, Sperber writes "there was something sad . . . about this spectacle of the revolutionary intimidation and terrorization of some of the poorest and most wretched inhabitants of the Palatinate" (p. 444). And so there was. I readily confess to being unable to view the political turmoil of the early nineteenth century in a very positive light. I see much irony, some humor, and a few examples of great individual courage. More often, I see tragedy—pain, anguish, deception, and, yes, stupidity, too. Sperber sees democratic traditions. *Chacun à son coeur*.

KARL WEGERT
Bishop's University
Lennoxville, Quebec

JONATHAN SPERBER REPLIES:

Karl Wegert's letter, attempting to refute my account of his book, largely serves to confirm it. Wegert talks of his "lengthy opening 'Note on Sources,'" but no matter what principles for handling sources he asserts in the one page of remarks he devotes to this issue, his practice in handling sources is characterized by a refusal to confront ex post facto accounts given in judicial interrogations with evidence from the time of the action to which these accounts refer, an unwillingness to consider the often considerable axes the investigating judicial authorities had to grind, and a persistent acceptance of evidence supporting his thesis and rejection of evidence questioning it. As for the last part of his letter, which consists of contrasting adjectives from his book with adjectives from mine, this demonstrates all too clearly Wegert's confusion of labeling with analysis, in other words his penchant for name calling. Under these circumstances, I see no reason to change in any way my judgment of Wegert's work.

JONATHAN SPERBER
University of Missouri,
Columbia

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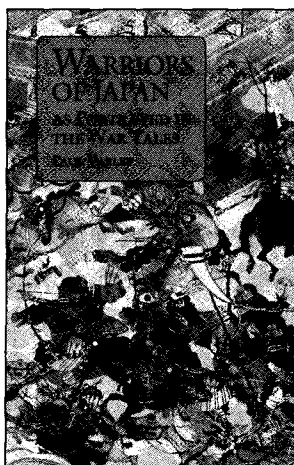
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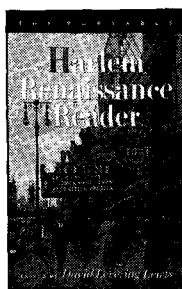
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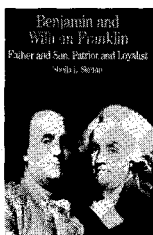
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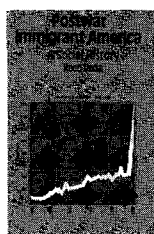
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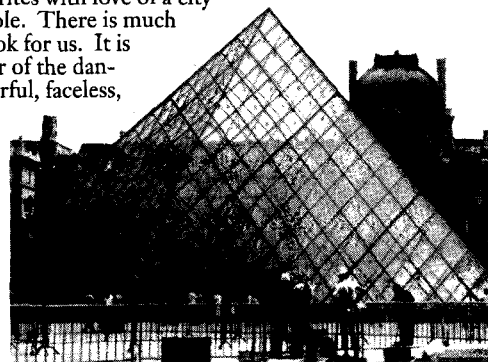
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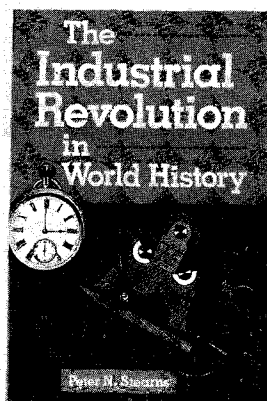
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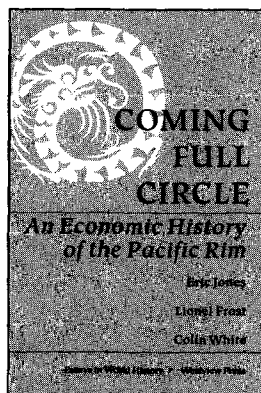
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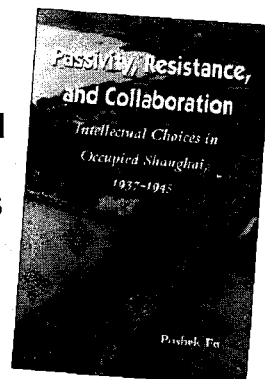
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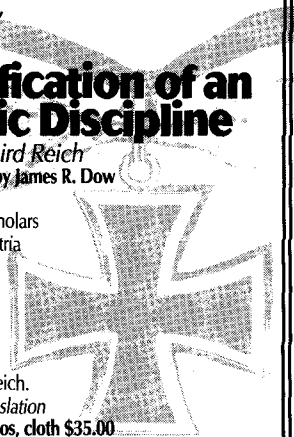
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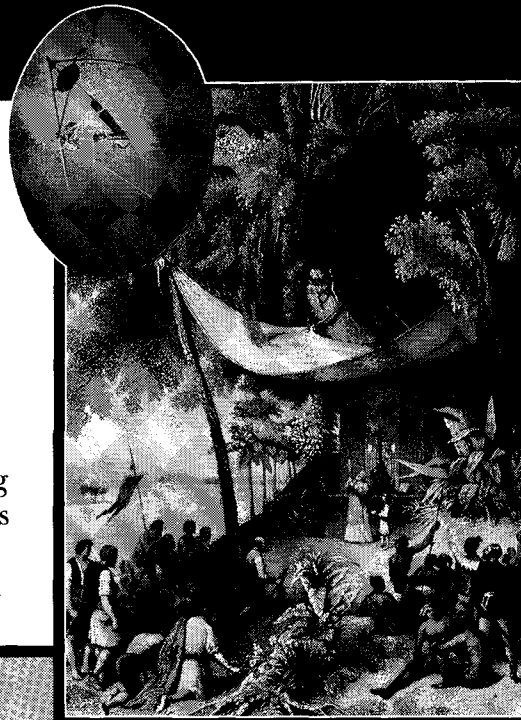
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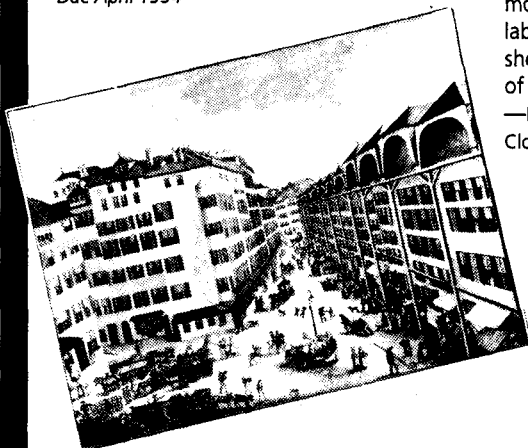
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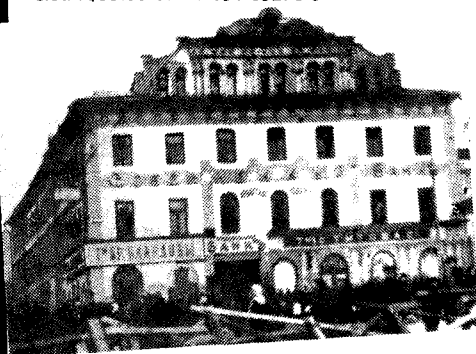
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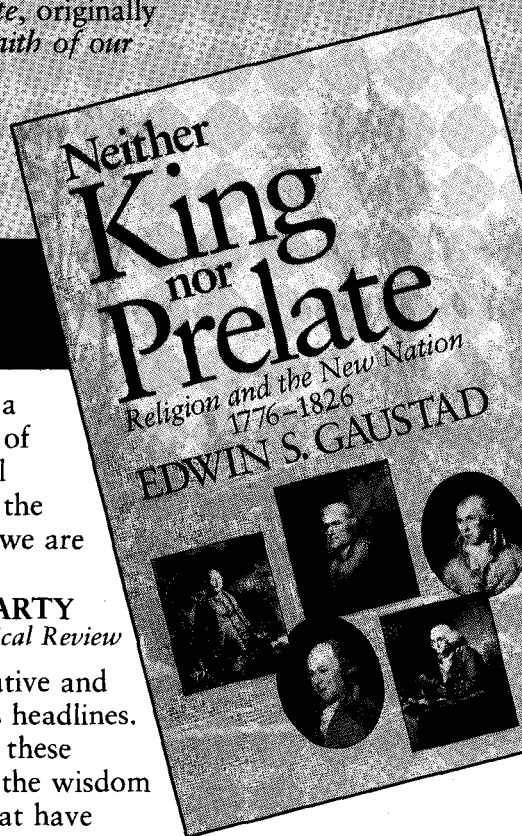
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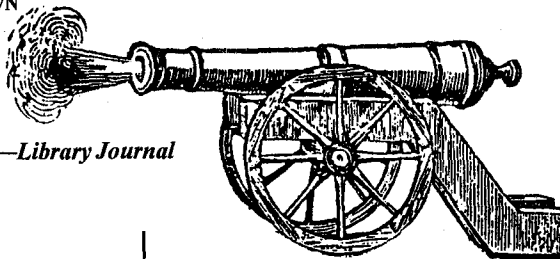
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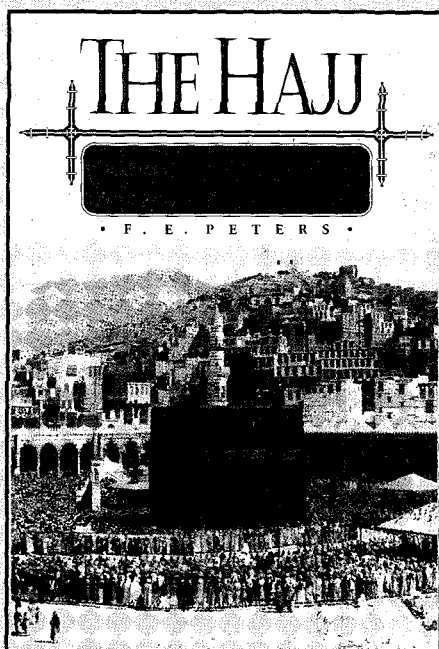


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